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AN OPTIMAL TECHNIQUE
FOR THE ALLOCATION OF FUNDS IN
R & D PROGRAMS

David Paul Greeneisen

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AN OPTIMAL TECHNIQUE
FOR
THE ALLOCATION OF FUNDS
IN
R & D PROGRAMS

A dissertation submitted in partial
fulfillment of the requirements
for the degree of

MASTER OF BUSINESS ADMINISTRATION
to the faculty of the department of
EXECUTIVE MANAGEMENT

at

St. John's University
New York

by

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Submitted

Approved

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Date

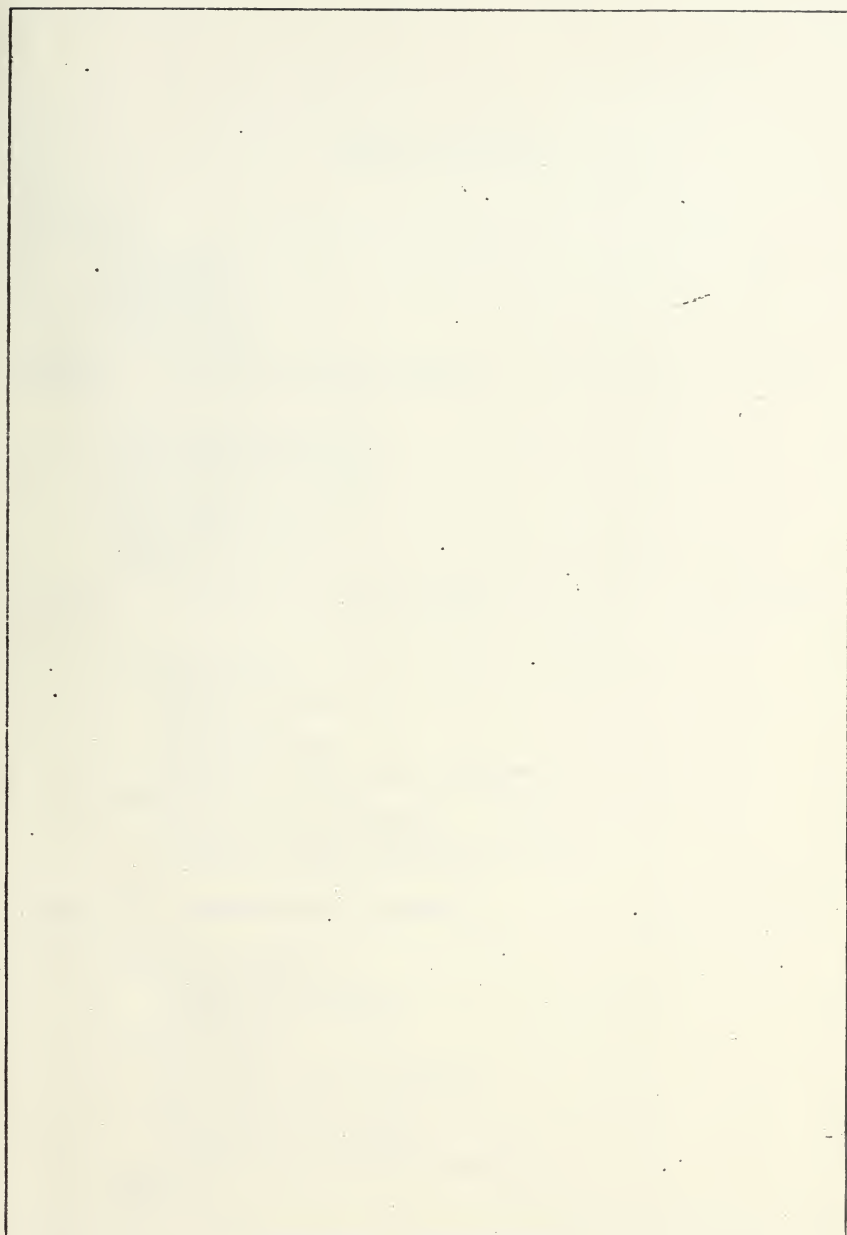


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INTRODUCTION

The Subject

The controversy surrounding the procurement of the C-5A Galaxy, a new transport aircraft obtained for the U.S. Air Force, has become a *cause celebre* for Congressional and Public critics of Department of Defense spending. The occurrence of exceptional cost over-runs in this program has been cited as evidence of poor management practice. One cannot refute this contention, for great excesses in spending above budgeted levels per force demonstrate poor management. However, it is important to determine which management techniques have failed, if circumstances such as those experienced in the Galaxy program are to be avoided in the future.

There is little doubt that the Galaxy program stretched the state-of-the-art, to a degree not originally anticipated, in aeronautical engineering and manufacturing, when it sought to reach new technological plateaus. It is now evident that economic, as well as technological factors, contributed to unforeseen developments. The initial planning estimates, made in 1964 and based on a preceding five-year period marked by a relatively stable economic environment, would certainly have been revised upward if conditions experienced during the succeeding five years had been anticipated.

Though the Galaxy program did experience certain technological and economic consequences which might be regarded as extraordinary,

it has not been unique in terms of unanticipated expenditures. It has been observed that "initial cost estimates of major weapons systems have historically escalated, just as they have in the C-5A."¹

One should not conclude from the preceding that problems in controlling cost increases are peculiar to government. Much of the unexpected expense for the Galaxy had to be absorbed by Lockheed Aircraft Corporation, the prime contractor, since initial contracts, based on Lockheed estimates, did not provide for the expenditures that were required. Corporations other than Lockheed have also experienced similar predicaments. For example, General Dynamics, while still trying to recover from the financial calamities experienced with the F-111 military aircraft and the Convair 880-990 commercial jets, is now encountering losses in shipbuilding programs; all due to unplanned cost.²

It is possible to gain some insight into the difficulties that have been described if recognition is given to the commonality which is displayed: each of the ventures which has been cited relied heavily on research and development to provide techniques and components needed for the achievement of program objectives. This fact would then lead one to conclude that a major contributor to problems of this type is a growing cost of technological advance.

Improvements in technology derive chiefly from research and development efforts. It has been observed, in this regard, that as technology does advance, the scientific disciplines, to be considered

¹ Armed Forces Management, (July, 1969), p. 58.

² "General Dynamics: In Trouble Again," Business Week, (October 4, 1969), pp. 48-52.

in seeking continued advance, become more esoteric in nature and greater in number. Thus, as program objectives move to successively higher levels of sophistication, the number of alternatives available for realization of the objectives increases, as does the costs of the alternatives.

The problem, then, is one which has increased in significance in the face of technological advances, and derives from difficulties encountered in both selecting appropriate courses of action for achieving objectives and in properly forecasting and controlling the expense to be realized in pursuing the selected course. It has been recognized that there is a growing need for improved techniques to be made available to managers for the purpose of making hard choices in the funding of research and development efforts.¹

The Objective

A major facet of the research and development project manager's role is essentially one of control. This is exercised by monitoring project progress and ensuring that achievement and associated expenditures are kept within planned limits. This function will also include the task of affecting certain modifications, as necessary, to correct deviations from the plan.²

¹Marvin J. Cetron, "Technological Forecasting: A Prescription for the Military R & D Manager," Naval War College Review, XXI (April, 1969), p. 14.

²William E. Souder, "Experiences with an R & D Project Control Model," IEEE Transactions on Engineering Management, EM-15 (March, 1968), p. 40.

Control of research and development is, of course, closely related to the planning function which entails the selection of programs and the scheduling and allocation of funds. Control and planning interact principally when information, obtained through control, causes changes in program scheduling or fund allocation. For instance, the progress realized in a program may result in that program being terminated entirely, or being expanded at the expense of some other program.

Difficulties are encountered in research and development management which are due to an intrinsic uncertainty. Frequently it is impossible to discern whether a program is on the verge of failure or success. This uncertainty is also quite prevalent in program selection since *a priori* estimates are often little more than educated guesses, and the selection of programs on the basis of merit is often difficult.

The objective of this paper is to develop a technique for evaluating the relative value of proposed programs, so as to assist in program selection; for monitoring program progress and evaluating observed deviations; and for providing information to be used in adjustment of original planned program content and expenditure levels. Proposed programs are to be assessed on the basis of some selected measure of utility so that available funds may be allocated among alternative proposals in order to realize a maximum return. Program progress is to be measured in terms of achievement versus cost and achievement versus time. Thus, it will be possible to identify programs that either fail to meet prescribed time schedules or exceed

budgeted expenditures. Finally, using data obtained from the analysis of program progress, it will be possible to recommend changes in program mix in keeping with observed results.

As was stated, it is intended that the technique, which is to be developed, will be useful only in selecting from among proposed programs. It is not intended to formulate proposals. The actual formulation of proposals must be done on the basis of objectives and applicable policy considerations. This requires a certain amount of subjective evaluation. Only after this evaluation has been completed, and acceptable candidate programs selected, is it possible to conduct an objective analysis on the basis of the parameters mentioned above.

In order to facilitate the development of an analytic technique, it will be necessary to make certain assumptions regarding the structure of subject programs, the statistical independence of programs and the probabilistic character of research and development efforts. However, necessary assumptions will be examined to ascertain the significance of any inaccuracies which may be introduced.

The technique which is to be generated will derive from management systems common to Department of Defense research laboratories. As a result, it will not be universally applicable in all management situations. However, since the intent is to simply demonstrate a means of incorporating certain analytic methods into the management process, the methods themselves should be easily adaptable to other management systems. Throughout the ensuing analysis, appropriate analogy will be made, as required, to assist in relating the discussion to non-defense oriented circumstances.

Definitions

This paper constitutes a merger of several disciplines which are of different nature and utilize a variety of nomenclatures. Since the discussion is foremost intended as a business-oriented work it is assumed that the reader is familiar with the terminology associated with management science and economics. Moreover, due to the essential quantitative character of the paper, it must be presumed that there is also some familiarity with the semantic peculiarities of mathematics; particularly probability theory and statistical analysis. However, certain terms which lack precise meaning, and are used frequently in this paper, do deserve definition as follows:

1. research and development.--activities which seek to advance the state-of-the-art in any technological discipline. The term research is usually associated with those activities concerned with basic concepts and theories and which devote considerable effort to identifying and defining topics not previously studied. Development, on the other hand, is generally considered to include those activities directed at refining and extending knowledge of existing topics. Research and development are considered together since research prompts development and since both are endeavors which, presumably, have not previously been conducted.

2. project.--any organized research and development activity, conducted by a single group and having defined objectives. A project is the lowest level of research and development organization, and cannot be decomposed into other activities which are capable of satisfying project objectives.

3. program.--a research and development effort composed of several projects. A necessary characteristic of a program is that its objectives can be satisfied even if one or more of its constituent parts, the projects, are discontinued.

The Procedure

Chapter I, "A Review of Current Methods," presents a survey of efforts by other authors in developing analytic procedures for use in planning and controlling research and development programs. The purpose of this survey is to describe the attempts that have been made in the past and to evaluate the degree of success that has been realized for the purpose of identifying difficulties which have been encountered. This survey is also intended to provide a general introduction to the elements of quantitative management methods.

Chapter II, "Description of the Technique," examines in depth the two principle components of quantitative research and development management systems; project selection and control. This description is in the form of an examination of various techniques that have been developed in the past for use in selection and control. This chapter forms the foundation for the techniques to be originated in this paper.

Chapter III, "Construction of the Model," presents the derivation of the selection and control techniques which are the subject of the paper. The derivation demonstrates a method of organizing elements of the procedures discussed in Chapter II such that an improved approach to the problem of project selection is obtained. Also, a technique for optimizing project selection is presented. The chapter

concludes with the development of a method for controlling research and development projects that is based on statistical inference.

Chapter IV, "Conclusions," summarizes the status of quantitative selection and control techniques as it stands at present, and reviews the role that is anticipated for the techniques offered in Chapter III. In addition, elements of the subject techniques which deserve further refinement are discussed.

Appended to the text, in Appendix A, is a sample of materials which are considered useful in the implementation of quantitative selection methods. Appendix B presents a tabulation of the cumulative Poisson distribution which is used in the control technique of Chapter III. Finally, Appendix C presents a partial tabulation of the chi-square distribution, also referred to in Chapter III.

CHAPTER I

A REVIEW OF CURRENT METHODS

Focusing On the Problem

As described in the introduction, the problem under consideration is one of developing more effective methods for the management of research and development programs. A specific provision was included which precludes processes involved in the formulation of programs. These processes, which are generally classified under the heading of normative forecasting, have to do with the identification of goals and selection of candidate programs on the basis of policy criteria; usually excluding considerations of economy and technological feasibility.

After the normative forecasting process is complete, and a set of acceptable research programs has been generated, management then tries to determine which programs from the set to pursue (project selection), how much money to spend on each program (budget allocation), and the size of the total budget for all the programs selected (budget determination).¹ The first two of these functions, project selection and budget allocation, constitute the key areas of interest for this paper.

As was stated earlier, project selection is essentially a

¹E. M. Rosen and W. E. Souder, "A Method for Allocating R & D Expenditures," IEEE Transactions On Engineering Management, EM-12 (September, 1965), p. 90.

planning activity. However, budget allocation is a planning problem also; it takes place at the start of a program or at a review point where some change of plans is indicated. The key decisions in budget (or resource) allocation involve the rates of resource expenditure and the planned dates for the commencement of an activity.¹

It should be re-iterated that, since attention is going to focus on planning and plan review, consideration is appropriately due the associated control processes. In this respect, examination of management processes will include those concerned with monitoring program progress and ensuring that prescribed limits are observed.

The Analytic Framework

In the abstract, one can consider research as simply the purchase of information upon which to base later and better decisions. All but one of these decisions is whether or not to continue purchasing information (that is research). The one exception is the final decision, which is whether or not to commercialize the results. The decisions to continue a research effort, and the subsequent decisions concerned with how to conduct the research do appear to be sequential in nature. This sequential decision making process has been the subject of descriptive work in the past.² It is worthwhile to note that the applicability of

¹R. S. Rosenbloom, "Notes on the Development of Network Models for Resource Allocation in R & D Projects," IEEE Transactions on Engineering Management, EM-11 (June, 1964), p. 62.

²D. L. Marples, "The Decisions of Engineering Design," IRE Transactions on Engineering Management, EM-8 (June, 1961), pp. 55-71. and T. A. Marshak, "Strategy and Organization in a System Development Project," The Rate and Direction of Inventive Activity (Princeton, New Jersey: Princeton University Press, 1962), pp. 461-475.

the sequential decision model for research efforts is a function of the degree of structure that is evidenced in the effort. For example, in basic research, which is fairly unstructured, the model should be quite appropriate. In developmental design, however, decisions to commence a phase of the program may be made before preceding phases are complete. In this case, then, some inaccuracies may be introduced. Nonetheless, for the purposes of the present discussion, the model can be presumed adequate.

The concept of sequential decisions is only one ingredient in a characterization of research and development processes. Previous studies, for instance, have identified certain parameters as being significant in research efforts. Among these are the magnitude of total expenditures, duration, work scope, activity content, geographic dispersion of related elements, and others. Furthermore, any one or more of these parameters may be selected as a primary measure; depending on the type and objectives of the analysis for which the characterization is intended. Of the numerous parameters which might be used, certain ones have been found to be more useful as constituents of a model for the research process. These are:

- 1) the items worked on,
- 2) the activities (operations) performed on these items,
- 3) the duration of each assignment,
- 4) the effort (or resources) utilized by each assignment.

Furthermore, the dynamics of the research effort have been found to be adequately represented by the following:

- 1) the start and stop time of each assignment, in calendar time,
- 2) the total effort content of each assignment.

- 3) the distribution of this effort content over the duration of the assignment.¹

A final aspect of the research process which should be considered in an analytic characterization is the representation of the economies of operation. Here, economy of operation relates to the rate at which resources can be effectively employed. In this respect, the research process is partitioned into three funding stages. The first stage lies below a "critical" cost level, such that any allocation below this level yields no return. Above the critical cost level is a stage, within which, progress is a monotonically increasing function of cost. This mid-stage, which can be called the "adequate" stage, has an upper termination at a funding level called the "satiation" cost. Above this cost level increases in cost have no corresponding increases in progress.²

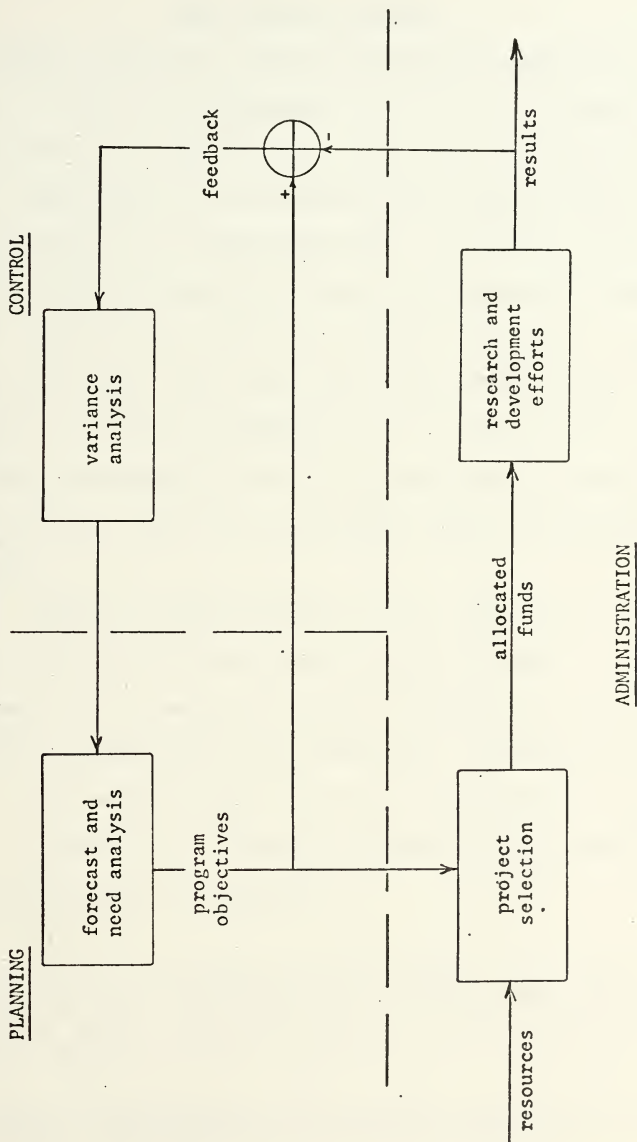
In concluding this discussion of the analytic approach to research and development planning and control, it is considered that the interactions of the several components of the management system can be summarized by graphic display as shown in Figure 1. It is intended that this should illustrate the inter-related nature of planning and control, and indicate the fashion in which each associates with program operation.

¹P. V. Norden, "On the Anatomy of Development Projects," IRE Transaction on Engineering Management, EM-7 (March, 1960), p. 37.

²R. J. Freeman, "A Stochastic Model for Determining the Size and Allocation of the Research Budget," IRE Transactions on Engineering Management, EM-7 (March, 1960), p. 4.

FIGURE 1

DIAGRAM OF MANAGEMENT PROCESS



Source: Original

Some Current Techniques

Numerous authors have offered quantitative techniques for the management of research and development.¹ Of these, the techniques that have objectives similar to those of this paper are described below:

1) Asher, 1962.--This method was derived for the purpose of man-power allocation in a pharmaceutical firm. The method considers a number of alternative projects on the basis of discounted net value, if successful; probability of success; and man-hours (resources) required, including skill levels. From these data an expected discounted value is computed. Next, linear programming is used to assign various research teams, characterized by skill levels represented, to maximize the expected return subject to a constraining number of hours available for each team.²

2) Atkinson and Bobis, 1969.--This is a method for determining money to be spent on product oriented research programs. The method commences by estimating the density function of the probability of a project succeeding for a given expenditure. This distribution is given in the form of a logistic function. On the basis of this function, the probability of completing a program within a given time, for a given expenditure, is determined.

¹For a brief but thorough review of current techniques, including an exceptional bibliography of the topic see M. J. Cetron, J. Martino and L. Roepcke, "The Selection of R & D Program Content--Survey of Quantitative Methods," IEEE Transactions on Engineering Management, EM-14 (March, 1967), pp. 4-13.

²D. T. Asher, "A Linear Programming Model for the Allocation of R and D Efforts," IRE Transactions on Engineering Management, EM-9 (December, 1962), pp. 154-57.

The next step in the analysis involves a determination of commercial value as a function of completion time. This, in conjunction with estimates of completion time yields an expected commercial value. Having these data, it is then possible to optimize the expected value of research on the basis of annual expenditures for the alternative programs being considered. Due to the formidable nature of this optimization problem which results from the numerous variables involved, an iterative optimization technique is employed.¹

3) Dean and Hauser, 1967.--The employment of this method commences with a decomposition of the research program into the following categories:

a) Qualitative Materiel Development Objectives (QMDO)--the several major goals of the research (effort); corresponding approximately to program objectives as defined in the Introduction.

b) Materiel Concepts--the various different component efforts of the QMDO (for instance, if a QMDO is the development of a missile, these might be guidance, propulsion and structure); corresponding approximately to program content.

c) Technical Approaches--the several alternative methods of achieving a materiel concept; corresponding to different projects which satisfy the same objectives.

The utility analysis of the research program is affected by first estimating the probability of success and cost for the technical

¹A. C. Atkinson and A. H. Bobis, "A Mathematical Basis for the Selection of Research Projects," IEEE Transactions on Engineering Management, EM-16 (February, 1969), pp. 2-8.

approaches. With these data it is then possible to determine the allocation of funds, among the technical approaches for a given material concept, which maximizes the probability of the material concept succeeding; subject to the constraint of a fixed amount being available for the material concept funding.

Assuming, that, in order for a QMDO to succeed, all component materiel concepts must succeed, it is then possible to find the funding levels for materiel concepts in order to maximize the QMDO probability of success; subject to a budget constraint.

This particular method has been found amenable to optimization by dynamic programming. The authors maintain that such a solution contributes flexibility and speed to solution, and is particularly suitable for the type of structure used in decomposition of the program.¹

4) Hess, 1962.--Hess introduced the first dynamic programming solution to the allocation problem. His solution assumes values for certain parameters, among them being the probability of technical success, expected profit as a function of time of completion, and an assumed measure of usefulness deriving from prior research. Using the dynamic technique that is developed, it is possible to obtain an optimal allocation of funds among programs and budgeted rates of expenditure for each program; subject to appropriate overall budget

¹B. V. Dean and L. E. Hauser, "Advanced Materiel Systems Planning," IEEE Transactions on Engineering Management, EM-14 (March, 1967), pp. 21-43.

constraints.¹

Aspects of Uncertainty

Complications attendant to uncertainty arising from operational situations are by no means limited to management of research and development activities. Similar problems are common to the management of most business efforts. This is evidenced by the development of disciplines such as statistical decision theory, and their rise to prominence in management science. Research efforts, however, are somewhat unique in that they demonstrate a degree of endogenous uncertainty not characteristic of other management processes. Even if environmental factors (such as market conditions and the supply of materials, labor and funds) were deterministic, the results of research and development programs would not be. Thus, the significance of uncertainty in research management has added importance.

As is indicated by the preceding description of some available techniques, this uncertainty is usually accounted for by means of a "probability of success" term. This approach to the problem, however, has some disadvantages.

To begin with, there is no universally accepted meaning of probability of success. Most authors account for this, to some extent, by allowing for a probability of technical success and, also, for

¹S. W. Hess, "A Dynamic Programming Approach to R and D Budgeting and Project Selection," IRE Transactions on Engineering Management, EM-9 (December, 1962), pp. 170-79.

commercial success. Even if one were to limit consideration to technical success alone, a probability measure is inadequate, since it does not allow for varying degrees of success which might result. In reference to the probability of success concept, Baker and Pound observed that "at best, it is extremely difficult to understand what is meant by such a term."¹

The use of probability of success is further complicated by the fact that it is extremely difficult to estimate a value for such a parameter. In the work done by Asher, for instance, probabilities ranged between five and twenty-five chances of success *per ten thousand*.² Needless to say, even the slightest variation in estimates, in cases such as this, would have disproportionate effects due to the small order of magnitude involved. There are two principle methods of estimating probability of success. First is by obtaining the subjective judgements of persons experienced in the subject area. Second is by analysis of data obtained from similar prior efforts. The main shortcoming of the first method lies in the bias that is likely to be introduced. The second is found disadvantageous since probability of success is not a fixed quantity, but is a function of the state-of-the-art and level of knowledge prevailing at the time of the estimate.³

¹N. R. Baker and W. H. Pound, "R & D Project Selection: Where We Stand," IEEE Transactions on Engineering Management, EM-11 (December, 1964), p. 129.

²Asher, "A Linear Programming Model for the Allocation of R & D Efforts," p. 155.

³Rosen and Souder, "A Method for Allocating R & D Expenditures," p. 87.

A final complication realized in using probability of success estimates is that there is no reason to assume that the probability of success and level of funding are independent. On the contrary, the converse is almost surely true. Despite this, only the method of Hess gives consideration to the apparent dependency.

Data Requirements

Since the descriptions of methods given earlier were somewhat abbreviated, the data requirements appear deceptively minor. In general all techniques, developed to date, require information of past efforts. As such, many of the methods are not applicable to organizations that are initiating research and development programs. In some cases, such as that of Hess, the data requirements and assumptions are quite restrictive. As Hess points out, a considerable amount of work would be required before his method could be applied.¹

The fact of the matter is that, at the present time, there is not sufficient data available upon which to base formally structured research and development selection decisions. Even if a great deal of past data were available, it is not clear how this might be useful in evaluating current, different projects; for reasons which have already been discussed.

As a result of this paucity of "good" information, compensatory methods have been undertaken. As was mentioned in the discussion of probability, one approach has been the use of subjective estimates, an

¹Baker and Pound, "R & D Project Selection: Where We Stand," p. 128.

approach requiring judgmental values for costs, returns, probabilities of success, and so forth. The difficulty with these methods has been that there is generally a prevalent bias which may render the data unreliable.¹ For instance, persons wishing to "look good" will consciously or unconsciously underestimate the potential of proposed projects. On the other hand, there are the incurable optimists who think they can produce results worth millions, at very low cost, and in a short period of time.² If anything, the most common tendency is towards over optimism. This, of course, is the error that is most costly.³

Optimization Criteria

As the term "project selection" implies, efforts of this sort seek to evaluate and compare alternative proposals for the purpose of selecting those identified as holding the most promise. It is obvious that this process requires a basis for selection. The most common criteria that are used are some form of profit or return on investment; either can be, and are, computed in sundry fashions. This limited view of what constitutes an adequate criterion has undoubtedly grown out of the tendency of business, in general, to use profit or return on investment. However, as Dean and Hauser have noted, until a method is formulated for application in a particular situation, the concern

¹Ibid., p. 125.

²R. J. Freeman, "A Stochastic Model for Determining the Size and Allocation of the Research Budget," p. 2.

³R. M. Anderson, "Handling Risk in Defense Contracting," Harvard Business Review, XLVII (July-August, 1969), p. 92.

should not be with the development of criteria, but with the formulation of models which are adaptable to criteria. The more general intent should be to generate methods which enable the manager to synthesize the information that is available, so that alternative criteria can be examined.¹

Progress to Date

Several studies have been conducted for the purpose of determining the extent to which quantitative techniques have been incorporated into program selection and budgeting efforts.² All of these have indicated that use of these techniques has been slight; despite the current trend of capitalizing on such methods in other management efforts.

One survey concluded that a major reason for the lack of significant employment has been that the methods have not been thoroughly tested, using real data.³ A more recent examination, completed during the year prior to this writing, observed that a more likely reason is that the rationale for the selections is not subject to external validation of any kind.⁴ In most cases, it was concluded, the only test of

¹Dean and Hauser, "Advanced Materiel Systems Planning," p. 22.

²Baker and Pound, "R and D Project Selection: Where We Stand," and Cetron, Martino and Reopke, "The Selection of R & D Program Content-Survey of Quantitative Methods."

³Baker and Pound, "R and D Project Selection: Where We Stand," p. 130.

⁴Robert Ayres, Technological Forecasting and Long-Range Planning (New York: McGraw-Hill Book Company, 1969), p. 198.

validity is the internal consistency and reasonableness of the comparison procedures. Such a criterion, however, is usually only fully satisfactory to the model designer.

One should not be too hasty, though, in discounting the utility of quantitative techniques simply because "they do not prove out". This, frequently, is a characteristic of any statistical decision-making technique.

Two conclusions might be made at this point. The first is that the problem at hand, that of project selection, and so on, may be deserving of some attention to developing methods of solution which are more practical in nature, vice those which are theoretically more precise. Second is a need to recognize that quantitative techniques should not be expected to replace the decision-maker. The impact of these techniques is not in problem solving, but in problem formulation; the way in which managers conceptualize their problems.

CHAPTER II

DESCRIPTION OF THE TECHNIQUE

The techniques to be discussed in this chapter are quantitative techniques similar to those examined in the preceding chapter. Before continuing, though, it should be recognized that techniques such as these are not used to any great extent in existing research and development management systems. Most systems, today, still rely on the basic methods of more conventional systems; being oriented towards control on the basis of budgetary variances.

The management of research and development solely on the basis of financial considerations, without a sincere attempt to incorporate progress reporting, in some instances degenerates into a contest among program managers, which tests political rather than technical skills. Several rather familiar methods of program management have been spawned under these conditions. One of these is the *squeaking wheel* method of allocating resources. Using this method, a manager, preparing for the allocation of funds, will reduce resources for all projects and then wait to see which project manager expresses the loudest complaint. On the basis of the most insistent squeaking then, resources will be restored until budgetary levels are reached.

A second popular management technique is that which seeks to perpetuate the *Glorious Past*. This method dictates that a group within

the organization, which has recently demonstrated notable success, should gain acceptance of research proposals for the next year, or five years, or for some other appropriate length of time. This doctrine, which is founded on the premise of "once successful, always successful" permits the pursuit of research, independent of usefulness or efficiency.

A final management approach to resource allocation is based on the *white charger* technique. To implement this method, management calls on various departments or groups within the organization to hold forth with well-rehearsed presentations complete with multi-color graphs and handouts. That group which provides the most impressive forensic display is then rewarded with increased resources.

Though techniques such as these seem to be tragi-comic in the context of management science, their popularity and frequency of use is substantial.¹ The fact that methods such as these are used to the extent indicated serves to illustrate the need for objective consideration of progress and achievement in research and development.

The process of program selection, regardless of the type of quantitative selection technique that is employed, must include certain operations and procedures. These necessary steps, common to all selection processes, constitute the subject material of this chapter. After these steps are examined and analyzed, the following chapter will describe the methods to be utilized.

¹M. J. Cetron, "Technological Forecasting: A Prescription for the Military R & D Manager," Naval War College Review, XXI (April, 1969) p. 29.

Project Selection

The process of selecting research and development projects is closely akin to most other decision-making processes in that it requires the evaluation of alternatives preparatory to actual selection. In this respect, then, project evaluation implies *a priori* technical evaluation to determine the degree to which projects contribute to the accomplishment of established goals and objectives. This is in contrast to a conventional meaning of evaluation, which connotes a post-mortem to analyze the effectiveness and efficiency of the conduct of an accomplished project. Project evaluation is, therefore, intended to originate some sort of a characteristic quantity or expression which represents a project and will enable a value comparison with other projects competing for funds and resources.

Objectives

Prior to the commencement of project evaluation, it is necessary to identify some broad objectives of the entire selection process. These objectives may simply be in a statement of guiding policies, or they may be more closely defined.

Research and development objectives are usually products of long-range planning. This planning process commences, as does planning for non-research and development activities, with consideration of existing plans and policies, the state-of-the-art in disciplines of interest and the competitive environment. The results of this

portion of the planning process can be considered to be "strategic objectives"¹ since they derive from consideration of current research and development conditions and focus on projected conditions and competitive position.

The nature of strategic objectives can be illustrated if one considers a hypothetical case: Cunisibe Metals, Inc.² is a medium-size materials research organization which limits its efforts to non-ferrous applications. In the past, Cunisibe has usually expended about 75 per cent of its basic research budget in the copper-bronze field. However, Cunisibe's position in this field has never improved substantially in the face of considerable competition from the research divisions of the nation's three largest copper-producing companies.

Cunisibe's most recent planning effort has high-lighted two particular environmental factors:

- 1) The questionable long-range position of copper-bronze materials due to recent shortages of domestic raw materials and an even more questionable stability among African suppliers.

- 2) A growing interest in ocean-engineering and efforts to exploit ocean-resources.

As a result of these factors, then, Cunisibe formulated a strategic objective aimed at gaining a national position based on an

¹The terminology used in this section regards strategic, management and operational objectives, will correlate with the terminology of R. N. Anthony in Planning and Control Systems -- A Framework for Analysis (Boston: Harvard University, 1965).

²This is a fictitious organization. However, the circumstances described resemble those of an actual research organization.

expertise in developing materials appropriate for use in the ocean environment. This strategic objective will cause Cunisibe to shift emphasis from copper-bronze research to endeavors in utilizing such materials as tungsten and magesium. Likewise, it will open new disciplines, such as galvanic-corrosion control.

Strategic objectives, however, cannot afford adequate guidance for immediate applications. They simply identify long-term needs. The next step, then, towards establishing operational requirements is to determine the nature of corporate deficiencies regards the established strategic objectives. This portion of the planning process is essentially a deficiency analysis of current capabilities and resources. The result of this analysis will be "management objectives" which will be applicable in short-term planning. These management objectives will identify subject areas deserving attention in organizing programs. Following the previous example, management objectives at Cunisibe for the first few years might include:

- 1) Acquisition of personnel with expertise in the fields of Tungsten alloying processes, scintered metallurgy and metal-fibre growth techniques.

- 2) Development of research facilities adequate to support new areas of interest.

An examination of management objectives would reveal that, though they are more precise than statergic objectives, they are too general to be immediately useful. Thus, it is necessary to translate management objectives into "operational objectives" which can be

directly related to particular research endeavors. These operational objectives are the criteria that are used in the comparative analysis of research proposals.

Operational objectives are usually couched in terms of certain technologies or topic areas which have been identified as being of interest. However, for production-oriented, commercial firms operational objectives may be related in more economic terms. This, naturally, results from the fact that such firms have objectives which, generally, are more readily expressed in terms of economic indicators. For example, one market-oriented firm adopted the following as the objectives of research and development programs:¹

- 1) To maximize expected profit.
- 2) To maximize expected research successes.
- 3) To achieve a return on expenditure (ratio of maximum expected profit to total expended) of at least 55.0.

Regardless of whether a research and development organization is profit oriented or non-profit, or devoted to product development or process research, the exercise of formulating objectives is essential to any attempts at selecting program content on the basis of structured, quantitative procedures. As will be described in following sections, these objectives are needed if candidate programs are to be compared in an objective fashion, and if ultimate program progress is to be monitored meaningfully.

¹Rosen and Souder, "A Method of Allocating R & D Expenditures," p. 90.

Utility Analysis

A significant characteristic of the process of formulating objectives, that has been described in the preceding, is that it is almost exclusively a qualitative exercise. Consequently, the decisions, made in arriving at objectives, have little utility in terms of a quantitative selection technique. This section will be devoted to describing some traditional operations research procedures which can be used to relate quantitative techniques to qualitative objectives.

In the preceding example the formulation of objectives progressed to the point of operational objectives, which are to provide the necessary decision rules for program selection. The next step in the analysis must then cast program alternatives in terms which are amenable to manipulation, in order that decision rules may be applied.

The quantitative characterization of alternatives is most easily done using "figures of merit" or "value functions". Either of these terms refers to the assignment of a number or expression, to each alternative, which represents the utility or desirability of the alternative. The measurement of utility in this fashion is an essential ingredient of traditional operations research methods such as game theory, allocation problems and solutions of competitive situations.¹ Though a variety of utility measurement methods are available, an examination of two of them will adequately illustrate

¹R. L. Ackoff and M. W. Sasieni, Fundamentals of Operations Research (New York: John Wiley and Sons, 1968), pp. 49-56.

the principles involved.

Utility analysis, seeks to determine the value or value function to be attached to each research proposal. Value, here, should be thought of as expressing the "net worth" of a proposal to the research organization. It has been mentioned previously, and deserves re-emphasis, that the measure of value is a function of objectives. Thus, the rules for value assessment must be known before assignment is made.

To illustrate the nature of the value assessment problem, consider the case of storage battery technology, and the problem of assessing the importance of three battery parameters: volume, cost and time between recharging. Assume that the importance of each parameter will be indicated by assigning to each a number ranging from 10, for great importance, to 0 for no importance. Further, assume that an assessment of importance, or value, will be made by each of the following:¹

- 1) the user; a Lieutenant, U.S. Navy, commanding a boat containing batteries and drifting on a Vietnamese river on night patrol.
- 2) the R & D manager; an Admiral, responsible for the Navy's total research and development program, while considering next year's budget.
- 3) the boat designer; a naval architect designing a boat for use in Vietnam.

¹M. J. Cetron, "Technological Forecasting: A Prescription for the Military R & D Manager," p. 29.

4) the R & D engineer; a project engineer working in a Navy laboratory and seeking to improve the general performance of batteries.

Obviously, each of these individuals will consider battery parameters from different points of view. It should not be surprising if the responses of the four individuals were as different as those shown in Table 1.

TABLE 1
BATTERY PARAMETER RATINGS

Parameter	User	Manager	Designer	Engineer
Volume	3	2	10	8
Cost	0	10	2	2
Time Between Recharging	10	2	4	1

If the operational objective of a battery development program were to provide the best battery for combat units, the value assessment of the user would likely be the most accurate. However, if the objective were to maintain some battery research and development despite budget cuts, the manager's assessment would be appropriate. Similarly, other objectives would lead to the selection of either the designer's or the engineer's assessment.

This example should serve to illustrate the important fact that research and development programs have no intrinsic value or worth. Value must be measured with consideration given to objectives.

The preceding example also shows a method of translating qualitative judgements into quantitative measures. Before conducting the exercise associated with the data in Table 1, the R & D engineer could have related that he considered battery volume to be more significant than cost or time between recharging. This observation, however, would be of limited use to someone selecting a battery on the basis of the engineer's criteria. After the process of rating the parameters on a 0 to 10 scale, though, one realizes that the engineer regards volume as being four times as important as cost and eight times as important as time between recharging. Now, a quantitative selection is possible.

Selection

The selection problem involving alternatives having n significant parameters takes the general form of¹

$$v_k = \sum_{i=1}^n w_{ki} = \sum_{i=1}^n a_i x_{ki} \quad (\text{II-1})$$

where v_k = the value of the k th alternative

w_{ki} = the value of the i th parameter of the k th alternative

a_i = the weight of the i th parameter

x_{ki} = the i th parameter of the k th alternative.

¹Freeman, "A Stochastic Model for Determining the Size and Allocation of the Research Budget," p. 2.

This type of selection can be demonstrated by considering three hypothetical alternative batteries; the parameters of which are shown in Table 2. Applying equation (II-1) to the data in Table 2 (the

TABLE 2

BATTERY PARAMETERS

Parameter	#1	#2	#3
Volume (ft ³)	.80	1.10	.95
Cost (\$)	110	80	87
Time Between recharging (hr)	16	12	14

data being normalized by the mean for each parameter), and using the weighting values given by the engineer in Table 2, one obtains

$$v_1 = 8 \times \frac{.80}{.95} + 2 \times \frac{110}{92} + 1 \times \frac{16}{14}$$

$$= 10.26$$

$$v_2 = 12.80$$

$$v_3 = 10.90$$

Since it is presumed that one would seek minimum cost, volume and time between recharging, the alternative having minimum value (v) would be selected; that is, battery number 1. It should be noted that the results of the selection process would have been substantially different had other criteria from Table 1 been employed.

The preceding discussion illustrates the essential characteristics of the selection problem. First, the problem involves numerous

alternatives; say n . Each of these has a present net value, V_i . Though not mentioned explicitly, before, each alternative will also have an associated cost, C_i . If one assumes that alternatives are independent, and that the V_i and C_i are sufficient estimates of actual values, then the general selection problem is to simply find

$$\max \sum_{i=1}^n V_i \quad (\text{II-2})$$

$$\text{where } V_i = f_i(C_i) . \quad (\text{II-3})$$

Unconstrained, the solution of this problem is trivial since it amounts to simply selecting all alternatives.¹

A constraint that is commonly encountered in selection efforts, is that which limits available funds, in the form of a budget. In this case, for T years in the planning period

$$C_i = \sum_{t=1}^T c_{it} \quad (\text{II-4})$$

where c_{it} is the cost during year t . If an annual budget, B_t , must be observed, then the solution is subject to

$$\sum_{i=1}^n u_i c_{it} \leq B_t \quad ; \quad t = 1, \dots, T . \quad (\text{II-5})$$

¹Engineering Economy Division, American Society for Engineering Education. The Fourth Summer Symposium Papers (Hoboken, New Jersey, 1966), p. 65.

where $u_i = 1$ if alternative i is selected,

$u_i = 0$ if alternative i is rejected.

Interdependence

The general selection problem, described by equations (II-2) through (II-5), can be expanded by admitting interdependence among alternatives. This provision would result in

$$V_i = f_i(C_1, \dots, C_i, \dots, C_n) ; \quad (\text{II-6})$$

that is, a given alternative may be affected by expenditures for other alternatives. The most common example of this situation arises when two or more different research projects are being conducted by the same working group; for example an organic chemistry branch. In situations such as these, though the research projects are different in substance and structure, by virtue of the mere fact they all relate to organic chemistry, it would be expected that some interdependence would exist.

As soon as the possibility of interdependence is admitted, the nature of the selection problem changes substantially. Though the objective of maximizing expected value, as expressed in equation (II-2) is still applicable, it is no longer a simple, or trivial problem. Recognizing the implications of equation (II-6), which can be rewritten as

$$\underline{V} = \underline{\phi} \underline{C} ,$$

where ϕ is the transition matrix, relating C to V , it becomes apparent that some techniques, other than simple rank-ordering, will be required to obtain a solution to this selection problem.

The first step in treating the consideration of interdependence is to estimate its quantitative characteristics. The first major attempt to do just this was made in Project PATTERN.¹ This project sought to examine the contribution of so-called "cross-support" among research projects, and to use the information, thus gained, in planning future research efforts.

In the PATTERN report, an example research program, a Strategic Manned Base Orbital System, was compared with three other related projects. The objective of this comparison was to determine the progress to be realized in the Orbital System research if success were achieved in any one of the other three projects. The results of the cross-support analysis are shown in Table 3.

The data in Table 3 relate to certain elements of the Orbital System program (for instance, elements 4, 32, 33, 106, and so forth). For each of these elements, the degree of correlation with the research objectives of the three other projects being examined are shown. For example, element number 123 of the Orbital System project had a .05 correlation with elements 1531 and 1532 of the Infra-red and Radar Ranging project. That is, if the objectives of the Infra-red

¹Aaron L. Jestice, Project Pattern, Presented to the Joint National Meeting, Operations Research Society of America, October 7, 1969 (Minneapolis, Minn.: The Institute of Management Sciences, 1964), p.15.

TABLE 3
CROSS SUPPORT ANALYSIS OF
STRATEGIC MANNED BASE ORBITAL SYSTEM

Manned Base Orbital System	Hi Scan Rate IR and Radar Ranging		Hi Scan Rate Laser Range and Angle Radar		Hi Scan Rate Radar (10-30 KMC) (1 MW Peak)	
Project	Project	Cross Support	Project	Cross Support	Project	Cross Support
4						
32					1752	0.10
33					1753	0.40
106						
121			1504	0.30		
123	1531 1532	0.05 0.05				
138						
286			1524	0.30		
316			1520 1525	0.20 0.20		
317			1529 1530	0.05 0.05		
586						
589						
633			1760	0.80	1759	0.08
636			1762	0.70	176.	0.05
637			1764	0.90	1763	0.06
638			1766	0.70	1765	0.05
649			1768	0.60	1767	0.03

Source: Jestice, Project Pattern, p. 15.

project are achieved, then .05 of the effort to achieve Orbital System objectives would be unnecessary.

The significance of cross-support, or interdependence, among research projects is readily demonstrated. For example, assume that the Orbital System project has been selected for funding. Now, consider the problem of selecting one additional program, from among the three alternatives shown in Figure 3. If it is further assumed that the objectives of the three alternatives are of equal importance, and that the independent costs are the same, then, obviously, one would select the Laser Range and Angle Radar project for funding. This is so because the actual costs will be substantially less than the independent costs due to the degree of interdependence indicated by the data of Table 3.

It should be noted that the measurement of cross-support, as developed for Pattern, is a technique for quantifying the interdependence of research projects by examining the direct relationships among the projects. Another approach to the problem of accounting for interdependence is to measure project relationship to some objectives other than the interdependent projects. A typical application of this type of analysis is to examine the contributions of a given project to a number of selected technological disciplines.

One method for measuring project content in this manner is that of PROFILE (Programmed Functional Indices for Laboratory Evaluation).¹ This procedure, developed for evaluation of military

¹Erick Jantsch, Technological Forecasting In Perspective (Paris: Organization for Economic Co-operation and Development, 1967), p. 228.

research projects, relates project content to various military missions. The type of analysis utilized in PROFILE is an analysis of program relevance to prescribed objectives.

Relevance evaluation is most easily described by constructing a matrix to display the form of the analysis.¹ As shown in Table 4, each of the n program alternatives is related to the objectives, α through v , by the significance numbers, s . In addition, each of the objectives is assigned a weight, q , which indicates its importance relative to the other objectives.

The desirability of research programs can then be measured by relevance numbers, r , which are obtained from

$$r_{ji} = \sum_{\alpha}^v q_{\alpha} s_{\alpha j} \quad (II-8)$$

To ensure the homogeneity of the selection logic, the data in Table 4 is normalized subject to

$$\sum_{\alpha}^v q_{\alpha} = 1 \quad (II-9)$$

¹Ibid., p. 221.

TABLE 4
RELEVENCE EVALUATION MATRIX

Objectives	Weights of Objectives	Program Alternatives						
		a	b	c	...	j	...	n
α	q_{α}	s^{α}_a	s^{α}_b	s^{α}_c		s^{α}_j		s^{α}_n
β	q_{β}	s^{β}_a	s^{β}_b	s^{β}_c		s^{β}_j		s^{β}_n
γ	q_{γ}	s^{γ}_a	s^{γ}_b	s^{γ}_c		s^{γ}_j		s^{γ}_n
\vdots								
χ	q_{χ}	s^{χ}_a	s^{χ}_b	s^{χ}_c		s^{χ}_j		s^{χ}_n
\vdots								
ν	q_{ν}	s^{ν}_a	s^{ν}_b	s^{ν}_c		s^{ν}_j		s^{ν}_n
		r^a_i	r^b_i	r^c_i	...	r^j_i	...	r^n_i

Source: Jantsch, Technological Forecasting in Perspective,
p. 221.

and

$$\sum_{j=a}^n s_{xj} = 1 . \quad (\text{II-10})$$

Summary of Evaluation Techniques

The process of selecting research and development programs, as it has been described, is one of sequential decision-making. However, too frequently, the sequential aspects of such an endeavor are not recognized by the research and development manager. These sequential characteristics are of prime importance in the description and structuring of the objectives and criteria to be used in project selection. The primary reason for selecting particular projects from those available is to maximize the ultimate benefit to the organization and to maintain some assurance that research efforts will be productive. As long as objectives, and the relations among them, remain unclear, it becomes doubtful that any selection method will achieve much success. A common failing of selection techniques lies in a disregard for proper planning and analysis in the formulation of selection systems.¹

The preceding description of project evaluation and selection principles has been presented in terms of the sequential decision-making process. Figure 2 displays the activities involved and the manner in which they are related. The description of the flow of

¹Baker and Pound, "R and D Project Selection," p. 131.

FIGURE 2
EVALUATION AND SELECTION PROCESS

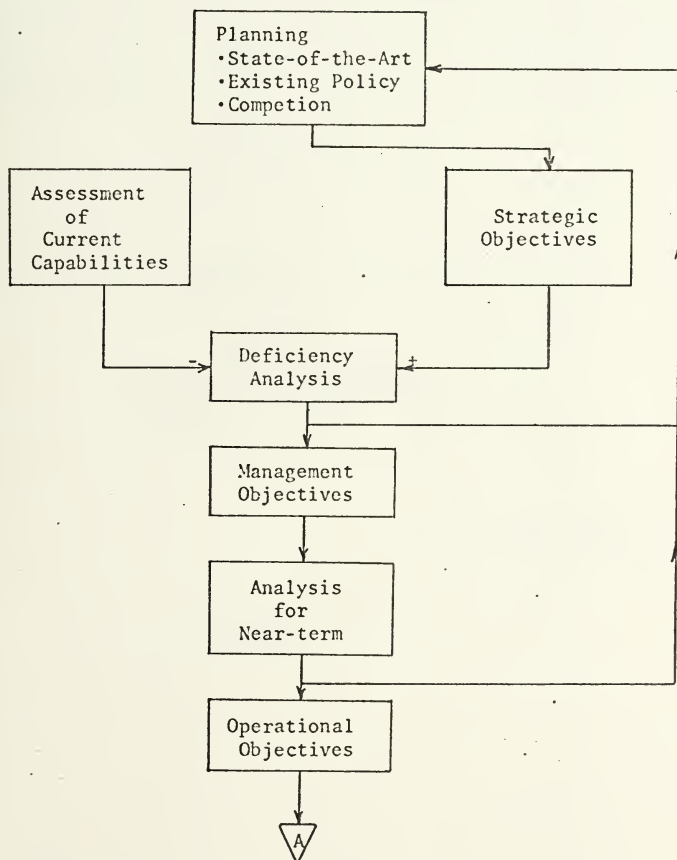
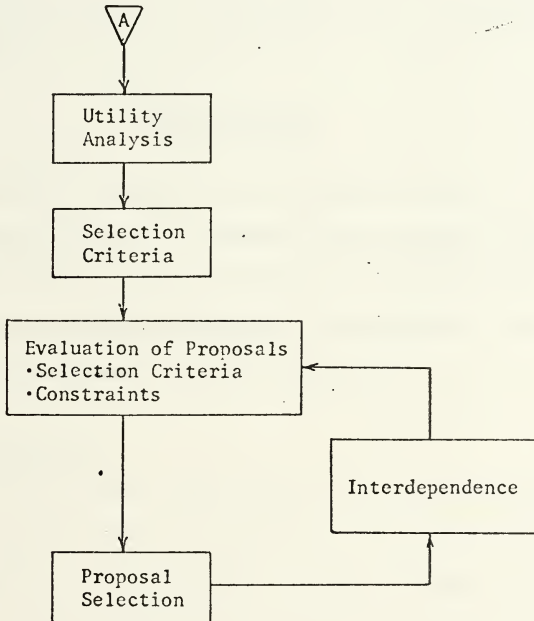


FIGURE 2--Continued



activity is intended to emphasize the serial nature of the process. It should be emphasized that, once operational objectives are established, no further feedback function is encountered until the evaluation/selection exercise. The significance of this lies in the understanding that modifying *objectives* for the purpose of making particular programs acceptable, defeats the intent of an organized selection effort.

Implementation

The selection of research and development projects is essentially a planning function. It commences with long-range plans and forecasts, and results in short-term plans for research efforts. The next step, then, in funding of research and development, is the process of implementing the selection decisions.¹

Time and Cost Parameters

The two most obvious characteristics of a research project, other than its subject matter, are the cost of the project and the time until completion. The importance of cost is obvious, for the undertaking of an organized selection effort is initiated by a need to allocate limited, financial resources among numerous candidate research efforts. Cost has added significance for those programs which span more than one budgetary period (usually a year). This results from the fact that monetary constraints are usually based on the dollar

¹Souder, "Experiences with an R & D Project Control Model," p. 40.

requirements of research programs for a given period, rather than the dollar requirements over the life of the project. In this regard, then, the research and development manager is usually interested in the distribution of funds over the life of the project.

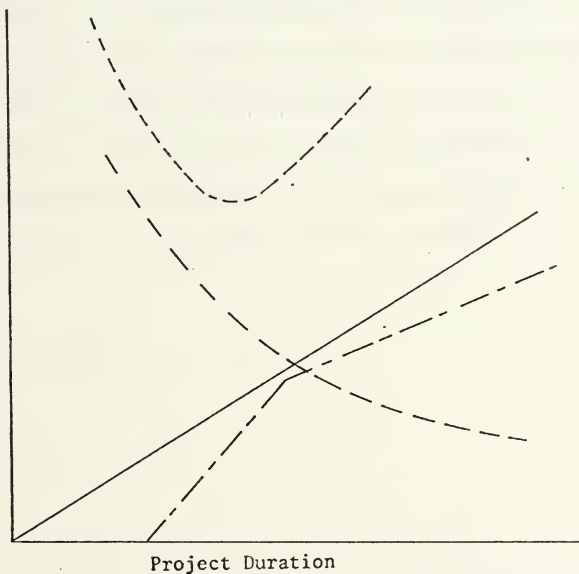
Project cost can be measured in numerous ways, and the method to be used is strongly a function of the nature of the organization conducting the research and development. In most commercial enterprises, for instance, a project incurs not only direct costs for material and labor, but also period costs and opportunity costs as indicated in Figure 3.¹

One example, however, of research and development funding which is not constrained by annual budgetary limitations, is government programs, which are the subject of this paper. Government research programs are generally selected on the basis of total cost with allowance made for the fact that appropriate annual fund appropriations will be forthcoming.

Another aspect of cost distribution, other than that concerning period budget constraints, addresses the question of efficiency in the rate of expenditure. At the root of this consideration is the fact that research achievement is not a linear function of funds expended. At times, research programs are funded on a "crash" basis, with available monies being increased considerably, in anticipation of realizing earlier success. Though project completion may be

¹Rosenbloom, "Notes on the Development of Network Models for Resource Allocation in R & D Projects," p. 60.

FIGURE 3
PROJECT COST FUNCTIONS



Key:
— Direct Costs vs. Duration
— Period Costs
--- Opportunity Costs
--- Total Costs

Source: Rosenbloom, "Notes on the Development of Network Models for Resource Allocation in R & D Projects," p. 60.

expedited in such a fashion, there exists an inherent wastage in this type of effort. This wastage will result primarily from less effective control and co-ordination and less efficient utilization of learning processes. Thus, wastage will occur when expenditures exceed some "satiation" level, as discussed in Chapter I.

One study, of the expenditure-achievement mechanism, found that achievement, as a function of expenditure, approximates an exponential function.¹ This study hypothesized that there exists, for the i th period of a program, some expenditure, y_i , which can be considered efficient. Then, if x_i is the actual expenditure for that period, the effective expenditure, z_i , or that expenditure which actually contributes to achievement, can be found from

$$z_i = y_i \left(\frac{x_i}{y_i} \right)^{\epsilon_i} \quad ; \quad x_i > y_i, \epsilon_i < 1$$

$$z_i = x_i \quad ; \quad x_i < y_i$$

where ϵ_i is a characteristic of the research project.

A final aspect of the study concluded that wastage, adhering to inflated expenditure rates, was more pronounced during the initial stages of a research program, and decreased in significance as the effort matured. Initial activity in research programs is generally devoted to seeking direction and definition. Consequently, added

¹Atkinson and Bobis, "A Mathematical Basis for the Selection of Research Projects," p. 5.

funds made available during this period will most likely be applied to parallel exploration. The results of these funds, ultimately, will not be germane to the research effort and therefore, must be considered wasted for purposes of the project. However, the acceleration of funding in the latter stages of a project will result in parallel endeavors in more closely defined areas, such as development, and will be more productive in terms of useful results.

Another study of considerable interest, in this regard, was that conducted by Hess, as described in Chapter I. As a result of this study, which examined the optimal expenditure for a given period of a project, and gave consideration to the history of preceding expenditures, Hess concluded that, in general, actual expenditure practices closely approximated the theoretical optimum.¹ One can conclude from this, that the prescriptive, or intuitive practices, which are still the foundation for most research and development funding procedures, are substantially adequate. Indeed, one study, which examined precisely the question of subjective forecasts and estimates by research and development managers, found that prescriptive practices are not unreliable, and the concomitant subjective forecasts can be extremely accurate.

From the foregoing, then, one must recognize that expenditure rates in research programs, that is the distribution of expenditures over the life of the program, is worthy of consideration. However,

¹Hess, "A Dynamic Programming Approach to R and D Budgeting and Project Selection," p. 178.

analytic techniques which address this problem are ultimately founded on subjective estimates from operating personnel. Further, there is every indication that subjective estimates of expenditure rate requirements are quite adequate.

The second significant parameter of a research project, that of time until completion, has two important areas of impact in the value of a project. First is the added direct cost due to delay. That is, as continuation of a project is experienced, the cost of maintaining the research team and providing services is encountered. Second, and probably more important, is the decreased utility of the results of the research due to the delay. In commercial enterprises this may be realized in diminished, competitive position. In military research and development programs, the results may be of even more significance, such as the diminished strategic position which has resulted from the delayed development of the F-111 fighter-bomber.

The immediate consequences of extended time in research, though, are usually subordinate to those resulting from extensions in expenditures. Most organizations operate within well defined limitations on financial resources. On the contrary, time available is of infinite supply.

Quantitative techniques devoted to the consideration of the time factor in research are usually limited to estimates of expected value; intrinsic or commercial. One study, for example, estimated that the commercial value of the product of research would decrease

by some factor, k , which is derived from projected sales patterns.¹ Therefore, if V_0 is the original estimate of commercial value, based on an assumed completion date, the value after n years of delay would be

$$V_n = V_0 (1 - k)^n .$$

Summary of Implementation Procedures

Time and cost in research programs are quantities that are easily measured and readily incorporated into conventional control procedures. These parameters, then, are the essential characteristics of programs which are considered in most selection processes and also form the basis for program control.

Despite the utility of time and cost as primary program characteristics, difficulties encountered in the administration of research and development programs indicate that these parameters may not be wholly sufficient for the purposes intended. To this extent, then, the following discussion will examine features which should be added to control procedures.

Achievement as a Parameter

The ultimate intent of any research and development effort is the realization of some process, procedure or product. The success of the effort is judged by a comparison of the actual results with

¹Atkinson and Bobis, "A Mathematical Basis for the Selection of Research Projects," p. 3.

those intended. Planning and control activities associated with research and development, assume, in the traditional manner, that given research efforts can be characterized by measures of time and cost which can be associated with the subject research and its intended results. As was discussed in the introduction, however, notable failures in the control of research programs have been experienced. This usually results from a lack of correlation between research achievement and estimates of associated time and cost.

The ineffectiveness of many control systems can be attributed to a disregard for the relationships among achievement, time and cost. It should be apparent that an effective control system must yield valid indications of the ultimate results of a research program by: informing the manager of any deviations from planned values of achievement, time or cost; indicating corrective actions needed; and communicating these indications to the manager. To this extent, then, an effective control system must merge achievement with time and cost. However, most control systems, utilized today, separate achievement reporting from cost and time accounting and, therefore, contribute to ineffective performance.¹

Numerous reasons might be suggested to explain the observed disinterest in achievement measurements or the difficulties encountered when such measurements have been attempted. One of these reasons is that it is frequently difficult to analyze achievement. For some development projects, achievement indices based on quality

¹Souder, "Experiences with an R & D Project Control Model" p.40.

and quantity of accomplishments can be devised. However, most research projects are not as easily handled because the significance of interim findings may not be known until the project is nearly complete.¹ Consequently, the basic mechanics involved in implementation are often the source of discouragement. Many of the difficulties encountered in attempting to quantify progress in research and development result from a literal-minded approach to the problem, which is reminiscent of conventional production-type operations.

As a result of this neglect of achievement in control systems an inordinate amount of significance is generally attached to project cost overruns. This, in turn, contributes to the introduction of unrealistic reporting of results, frequent neglect of projects which operate within the budget and the tendency of project managers to expend funds, regardless of need, in order to avoid budget reductions in following periods.

In summarizing the status of management science, as it applies to research efforts, two salient features warrant emphasis. First is the practice of selecting and funding projects primarily on the basis of *a priori* data; that is, on the basis of initial estimates of cost, time and utility of the product. This, however, is a peculiar anomaly when one considers the uncertainties inherent in research and development. The longevity of decisions regards selection and funding seems quite inappropriate in view of the tenuous nature of the informational basis for these decisions.

¹Ibid., p. 42.

The second feature of the process concerns the nature of the inaccuracies in data. The estimates required by conventional selection and funding procedures are based on a manager's ability to anticipate the performance of personnel in research efforts. However, in many research programs the nature of the forthcoming work is unknown. Therefore, it is virtually impossible to ascertain whether the talents and capabilities of a research team are appropriate for the task. Even though estimates of costs for equipment and salaries for a project may be reasonably accurate, one cannot determine, on an *a priori* basis, that these resources will be used efficiently and yield expected results.

CHAPTER III

CONSTRUCTION OF THE MODEL

Having examined the more important ingredients of the problem of program selection and control, attention will now be focused on the procedures which are advocated by this paper. The procedures and techniques, which have been selected for use, constitute the model which is the basis of the quantitative method. The two major components of the model, dealing with selection and control, are discussed separately for they are two relatively distinct operations.

Project Selection

The process of project selection, as the preceding discussion has related, and as is shown in Figure 2, involves the measurement of project utility with consideration given to established objectives, and taking into account the effect of interdependence among projects.

As was pointed out, quantitative techniques are not employed to any extent until the formulation of operational objectives. Consequently, this discussion will assume that preliminary planning has been accomplished to the extent of having established operational objectives. Therefore, what remains to be shown is the method of completing the selection process on the basis of these operational objectives, project utility, interdependence and certain aspects of

funding.

Utility Analysis

In selecting a quantitative method for utility analysis, one must first decide how interdependence is to be incorporated. This does not result from any particular theoretical requirement, but, is simply a starting point which facilitates construction of the model.

The preceding chapter discussed two approaches to interdependence; that used in PATTERN, which is based on the direct measurement of cross-support among projects, and that used in PROFILE, which uses indirect measurement methods. Either of these approaches are adequate for purposes of project selection. Therefore, the basic technique of the PROFILE approach is adapted since it is more easily implemented.

The PROFILE approach relates project characteristics to research objectives. However, in actual application situations at the laboratory level, the relationships between projects and general objectives may be confused by intermediate requirements. Moreover, relating project characteristics directly to general objectives, which are usually strategic and of managerial nature, does not correspond to the process described in the preceding chapter, and shown, graphically, in Figure 2. Therefore, the method developed here will utilize requirements prescribed at the laboratory level in accordance with operational objectives.

The process of relating research projects to research requirements, for the purpose of project comparison, requires that the

subject requirements be expressed in terms that are applicable to all projects; otherwise the continuity of the comparison would be destroyed. In order that this requirement of continuity may be realized, an added measure is interjected. This added measure is expressed in terms of scientific disciplines which relate to both projects and requirements.¹ The desired relationships are then obtained by describing requirements in terms of the scientific disciplines, or technologies, which must be supported, and by describing projects in terms of the technologies to which they relate. The method described is shown graphically in Figure 4.

To describe the process of relating projects to sciences, consider three projects, P_1 , P_2 and P_3 , which contribute, in various combinations to four sciences, S_1 , S_2 , S_3 and S_4 .² Next, assume that P_1 supports S_1 and S_2 , P_2 supports S_2 , S_3 and S_4 , and P_3 supports S_1 and S_3 . It is now possible to quantify these relationships by constructing a transfer matrix

$$\underline{A} = (a_{ij})$$

where

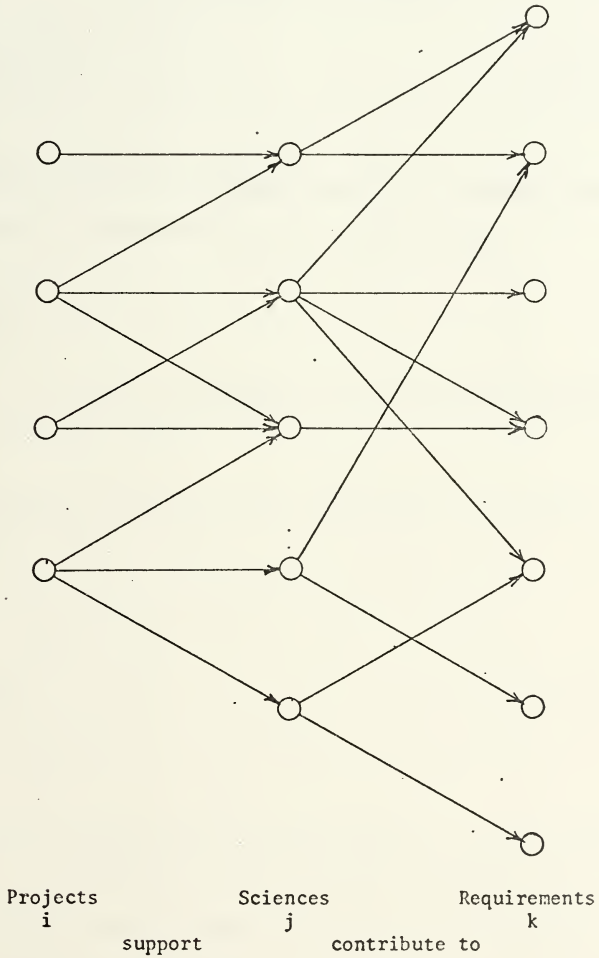
$$a_{ij} = \begin{cases} 1, & \text{if } P_i \text{ supports } S_j; \\ 0, & \text{if } P_i \text{ does not support } S_j. \end{cases}$$

¹B. V. Dean, "A Research Laboratory Performance Model," IEEE Transactions on Engineering Management, EM-14 (March, 1967), pp. 44-45.

²Ibid.

FIGURE 4

DIAGRAM OF PROJECT, SCIENCE
AND REQUIREMENT RELATIONSHIPS



Source: Dean, "A Research Laboratory Performance Model," p. 44.

If the project-science relationships are presented as in Table 5, it is apparent that

$$\underline{A} = \begin{vmatrix} 1 & 1 & 0 & 0 \\ 0 & 1 & 1 & 1 \\ 1 & 0 & 1 & 0 \end{vmatrix} \quad (III-1)$$

The relationships between sciences and research requirements can be similarly described using the transfer matrix

$$\underline{B} = (b_{jk})$$

where

$$b_{jk} = \begin{cases} 1, & \text{if a development in } S_j \text{ is required to satisfy } R_k; \\ 0, & \text{otherwise.} \end{cases}$$

If the science-requirement relationships are assumed to be those shown in Table 6, then

$$\underline{B} = \begin{vmatrix} 1 & 0 & 0 \\ 0 & 1 & 1 \\ 1 & 0 & 1 \\ 1 & 1 & 1 \end{vmatrix} \quad (III-2)$$

On the basis of the preceding analysis, it is now possible to determine the relationships between projects and requirements, which was the original intent. Recall that, if project i contributes to science j , then

TABLE 5
PROJECT-SCIENCE RELATIONSHIPS

		Sciences			
		S ₁	S ₂	S ₃	S ₄
Projects	P ₁	1	1	0	0
	P ₂	0	1	1	1
	P ₃	1	0	1	0

TABLE 6
SCIENCE-REQUIREMENT RELATIONSHIPS

		Requirements		
		R ₁	R ₂	R ₃
Sciences	S ₁	1	0	0
	S ₂	0	1	1
	S ₃	1	0	1
	S ₄	1	1	1

$$a_{ij} = 1.$$

Likewise, if a development in science j is necessary to satisfy requirement k , then

$$b_{jk} = 1.$$

Consequently, if project i ultimately contributes to requirement k , then

$$a_{ij}b_{jk} = 1.$$

Likewise, if either

$$a_{ij} = 0, \text{ or } b_{jk} = 0$$

then project i is not related to requirement k , and

$$a_{ij}b_{jk} = 0.$$

It should be observed that a given project can contribute to some requirement in more than one manner. That is, a project may support several different sciences, which each contribute to the same requirement. For purposes of selection, it is necessary to determine the number of ways in which each project does contribute to each requirement. If c_{ik} is identified as the number of ways in which project i supports requirement k , the c_{jk} can be computed from

$$c_{ik} = \sum_j a_{ij}b_{jk}.$$

If \underline{C} is identified as the matrix of c_{ik} , then

$$\underline{C} = \underline{A} \underline{B}$$

To illustrate the application of this analysis, consider the values of \underline{A} and \underline{B} given in equations (III-1) and (III-2). For these

values,

$$\underline{C} = \underline{A} \underline{B}$$

$$= \begin{vmatrix} 1 & 1 & 0 & 0 \\ 0 & 1 & 1 & 1 \\ 1 & 0 & 1 & 0 \end{vmatrix} \begin{vmatrix} 1 & 0 & 0 \\ 0 & 1 & 1 \\ 1 & 0 & 1 \\ 1 & 1 & 1 \end{vmatrix}$$

or

$$\underline{C} = \begin{vmatrix} 1 & 1 & 1 \\ 2 & 2 & 3 \\ 2 & 0 & 1 \end{vmatrix} . \quad (\text{III-3})$$

The significance of \underline{C} can be shown by an examination of some of the elements, c_{ik} . For instance,

$$c_{11} = c_{12} = c_{13} = 1$$

indicates that project 1 contributes to all three requirements in one way. The fact that

$$c_{23} = 3$$

however, indicates that project 2 contributes to requirement 3 in three different ways. This is, indeed, the case, via sciences 2, 3 and 4.

The structure of the model, thus far, and the method of analysis employed, enables the ready determination of project utility, to the extent that the utility is a function of the number or requirements supported. This, however, is seldom the criterion, for some requirements are usually more important than others. Indeed, this

would likely be the case when considering operational objectives.

When objectives or requirements are examined, it is fairly easy to rank them in an order of importance or desirability. This was demonstrated in the preceding chapter when the parameters of storage batteries were considered. Any of several rating techniques may be utilized, depending on the nature of the requirements being considered. Regardless of how requirements are assessed, the result will be a value function, V . Assume, for example that a research administrator had examined the research requirements R_1 , R_2 and R_3 , and rated each on a 0 to 10 scale, and that the results of his evaluation were

$$v(R_1) = 10,$$

$$v(R_2) = 2$$

$$\text{and } v(R_3) = 4.$$

In order to ensure the uniformity of the selection logic, these ratings must be normalized to satisfy

$$\sum_i v_i = 1.$$

After normalization, it is possible to determine

$$\underline{V} = (v_i) = \begin{vmatrix} \frac{5}{8} \\ \frac{1}{8} \\ \frac{1}{4} \end{vmatrix}.$$

Having determined the value of the research objectives or requirements, it is then a simple task to evaluate the research projects. Recall that \underline{C} , as shown in equation (III-3), relates projects to requirements in terms of the number of requirements supported. Having determined \underline{V} , one can then ascertain the value of projects, r_i , in terms of the values of the several objectives, v_i , from

$$\underline{R} = (r_i) = \underline{C} \underline{V}. \quad (\text{III-4})$$

This, for the example used previously is found to be

$$\underline{R} = \begin{bmatrix} 1 \\ 2\frac{1}{4} \\ 1\frac{1}{2} \end{bmatrix}, \quad (\text{III-5})$$

which shows the relative values of the three candidate programs.

Success in using analysis techniques, such as those employed in PROFILE, is dependent on the degree of orderliness achieved in the process of determining the possible combinations of project-science and science-requirement relationships. In order to illustrate an approach which has some likelihood of success, a method developed for the Naval Material Command, Washington, D. C., is presented in Appendix A.

Expenditure Analysis

After having computed the value of alternative projects, it is necessary to examine project funding so that a cost-benefit

comparison can be conducted. The primary interest, in this regard lies in the relationship between expenditure and achievement. When the research manager is contemplating the allocation of funds, this relationship is needed to determine the desired level of project funding.

Over time, the distribution of expenditures for research and development projects, is generally expected to be similar to one of two likely patterns.¹ The first typical pattern shown in Figure 5a, is the decaying exponential; approximated by

$$f(x) = e^{-\alpha x} . \quad (\text{III-6})$$

This type of distribution characterizes a project which commences with large initial outlays, with a subsequent decline in expenditures. The second typical pattern, shown in Figure 5b, represents projects which commence more slowly, usually in some small exploratory effort, and build up to some maximum expenditure which is then followed by decline.

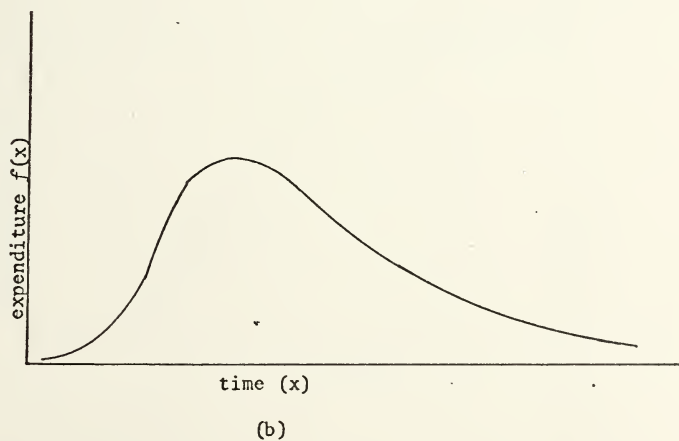
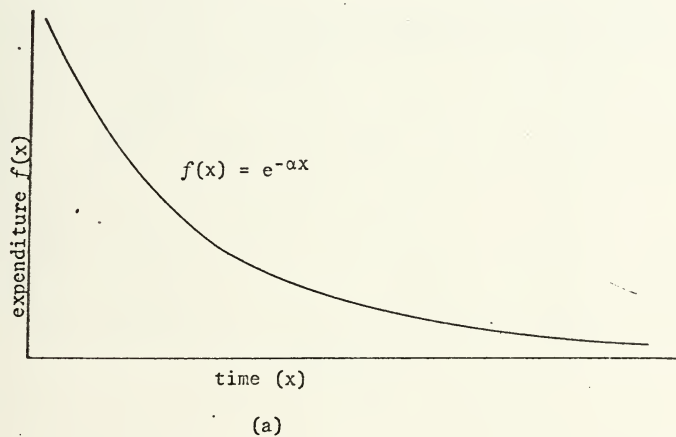
Although these expenditure distributions are of interest for budgetary use, it is the cumulative distribution which is of greater interest for purposes of project selection. As indicated in Figure 6a, the cumulative distribution, corresponding to the expenditure pattern of Figure 5a, is represented by a function of the form

$$f(x) = 1 - e^{-\alpha x} . \quad (\text{III-7})$$

Similarly, the distribution of Figure 5b relates to the general S

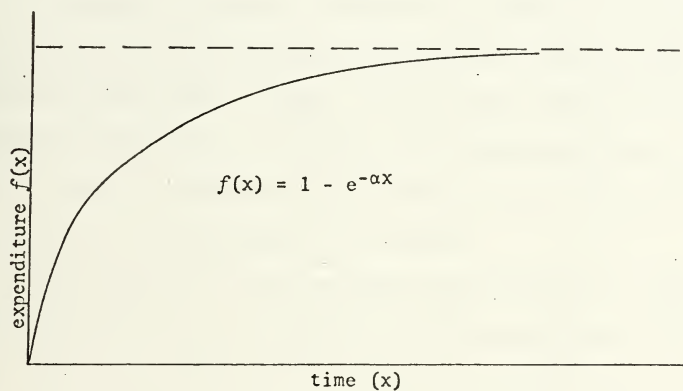
¹B. V. Dean and S. S. Sengupta, "Research Budgeting and Project Selection," IEEE Transactions on Engineering Management, EM-9 (December, 1962), p. 169.

FIGURE 5
TYPICAL EXPENDITURE DISTRIBUTIONS

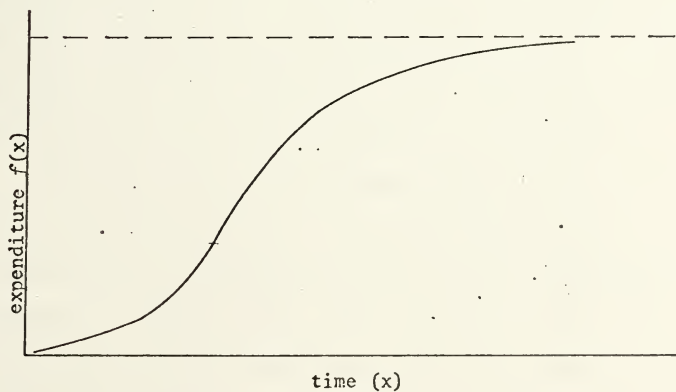


Source: Dean and Sengupta, "Research Budgeting and Project Selection," p. 169.

FIGURE 6
CUMULATIVE EXPENDITURE DISTRIBUTIONS



(a)



Source: Original; derived from integration of distributions in Figure 5.

curve shown in Figure 6b. The distributions, however, relate cost to time. What is actually of interest is the relationship between cost and achievement.

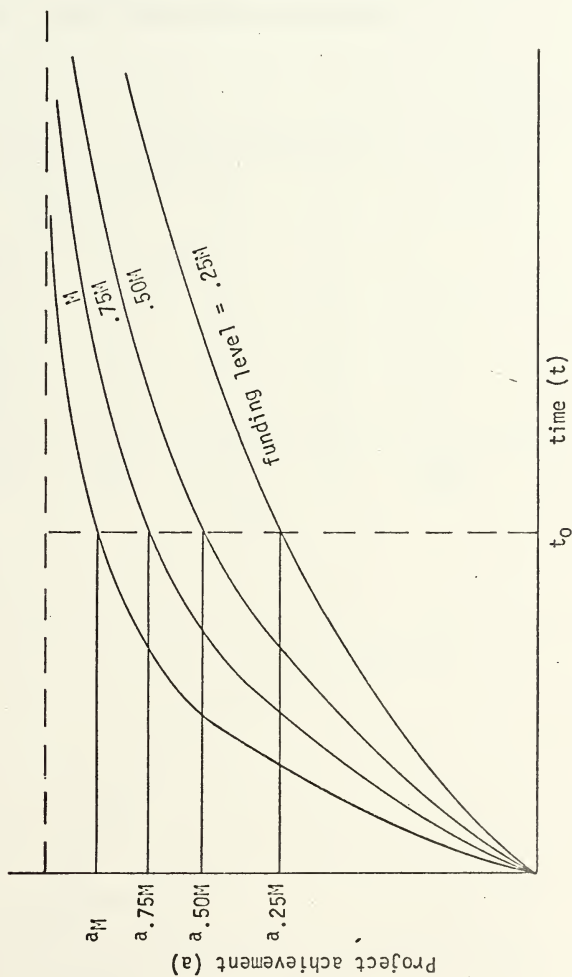
The impact on project achievement of variations in funding can be determined by considering the progress of a typical project for different funding levels. Figure 7 shows project progress for four different funding levels. The four levels illustrated commence with level 1, which is one-fourth of the satiation level, M , and increase in one-quarter increments until M is reached.

To determine the cost-achievement relationship, one simply takes a given time, t_0 , and compares cost-achievement points for that time. The results of such an examination are shown in Figure 8. As is indicated, for the achievement-time relationship of Figure 7, the resulting cost-achievement relationship is the familiar exponential as given in equation (III-7). However, the cost-achievement function, under other circumstances, may be similar to Figure 8b. The ultimate conclusion, then, is that achievement, as a function of cost, is similar to either the decaying exponential, or the S-curve.¹

Quantifying the cost-achievement function of research projects can be simplified even further. As shown in Figure 8, the two types of distribution are essentially the same above the achievement level corresponding to critical cost. Since management is not likely to be interested in funding levels below the critical level, the two

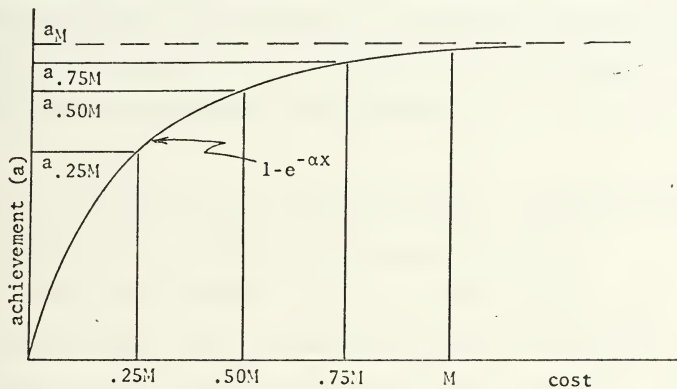
¹W. E. Souder, "Planning R & D Expenditures With the Aid of a Computer," Budgeting, March, 1966, p. 27, or Ayres, Technological Forecasting and Long-Range Planning, p. 177.

FIGURE 7
IMPACT OF FUNDING ON PROGRESS FORECAST

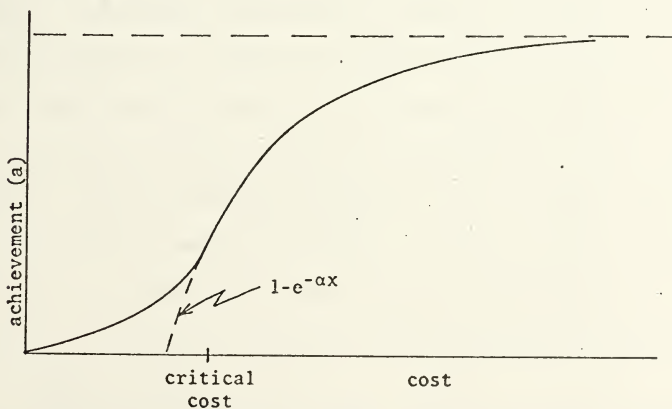


Source: original

FIGURE 8
COST-ACHIEVEMENT RELATIONSHIP



(a)



(b)

Source: Souder, "Planning R & D Expenditures with the Aid of a Computer," p. 27.

distributions can be modeled by the expression for Figure 8a, as given in Equation (III-7).

Having determined a form for cost-achievement, it is next necessary to find the parameters for the selected exponential. As would be presumed, the exponential is a function of the proposed fund allocation for project i , x_i . In keeping with convention, x_i is normalized by the function's time constant,

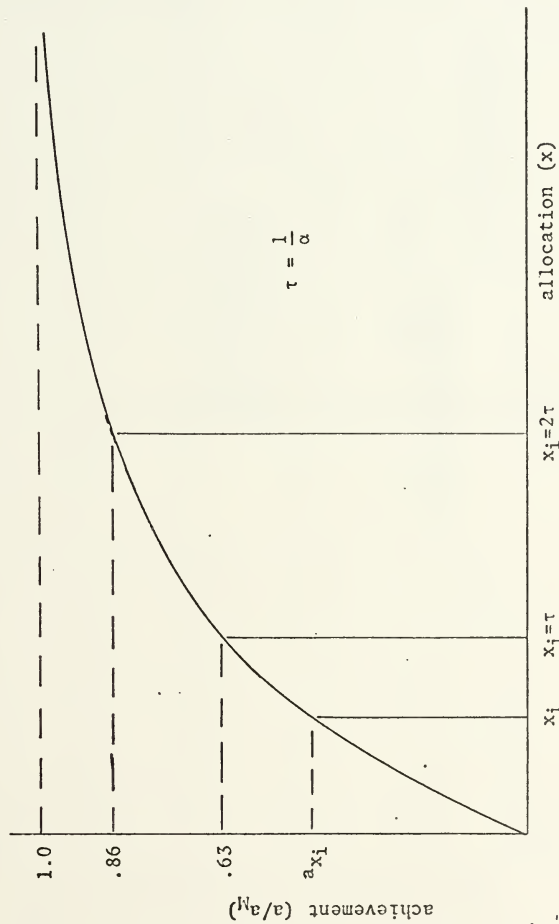
$$\tau = \frac{1}{\alpha},$$

which is that value of x_i at which 63 per cent of maximum achievement is realized. This notation is shown in Figure 9.

Since the functional form of cost-achievement has been uniquely specified, the relationship for any particular project can be found explicitly once any single data point is obtained. The procedure for this becomes apparent when one realizes that the 1.0 achievement level in Figure 9 corresponds to maximum (or unlimited) funding. Consequently, achievement is measured relative to the possible maximum. An achievement estimate can then be obtained, for a particular proposed expenditure. By examining a research forecast, it is possible to compare the expected advance in the achievement parameter, for the proposed expenditure, with the advance that might be expected from a maximum expenditure. Figure 10 shows the application of this method for a research project concerning high strength resins.¹ For this research project, then, an achievement-cost curve

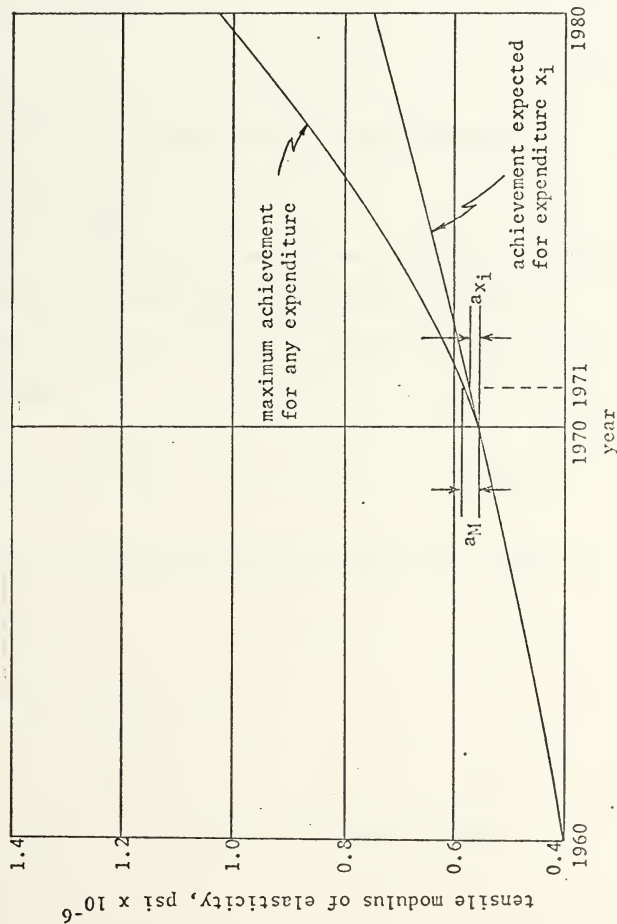
¹U.S., Department of the Navy. Naval Applied Science Laboratory, Technological Forecast in Depth: Organic Materials, 1968-1988, Lab. Project 930-100, Final Report (1968), p. 18.

FIGURE 9
COST-ACHIEVEMENT RELATIONSHIP



Source: Original

FIGURE 10

TENSILE MODULUS OF ELASTICITY
OF HIGH STRENGTH RESINS

Source: Derived from data in Naval Applied Science Laboratory Technological Forecast in Depth: Organic Material, 1968-1988, p. 18.

would be determined by plotting x_i on the abscissa, a_{xi}/a_M on the ordinate, solving for τ from

$$\frac{a_{xi}}{a_M} = 1 - e^{-\frac{x_i}{\tau}}$$

and completing the curve for the value of τ thus obtained.

Optimization of Selection

As the result of utility analysis and expenditure analysis, each project can be characterized quantitatively by combining equations (III-4) and (III-7) to obtain a profit function for each project in the form

$$p_i = r_i \left(1 - e^{-\frac{x_i}{\tau_i}} \right) \quad (\text{III-8})$$

The objective of the optimization procedure, then, is to maximize

$$P = \sum_i p_i \quad (\text{III-9})$$

subject to a budget constraint of

$$\sum_i x_i \leq B, \quad x_i \geq 0. \quad (\text{III-10})$$

Presuming that the intent is to expend the budgeted amount, this constraint becomes

$$B = \sum_i x_i, \quad x_i \geq 0. \quad (\text{III-11})$$

The task of optimizing equation (III-9) can be accomplished by using

the technique of the Lagrange multiplier.¹ To implement this technique, let

$$Q = P - \lambda B$$

$$= \sum_i \left[r_i \left(1 - e^{-\frac{x_i}{\tau_i}} \right) - \lambda x_i \right] \quad (\text{III-12})$$

where λ is some constant (the Lagrangian multiplier). Now, since Q differs from P by only a constant, λB , then, a maximum for Q will correspond to a maximum for P . To find the maximum for Q , compute

$$\frac{\partial Q}{\partial x_i} = 0$$

$$\frac{r_i}{\tau_i} e^{-\frac{x_i}{\tau_i}} - \lambda = 0 \quad (\text{III-13})$$

This is ensured to be a maximum, since

$$\frac{\partial^2 Q}{\partial x_i^2} = -\frac{r_i}{\tau_i^2} e^{-\frac{x_i}{\tau_i}} < 0.$$

From equation (III-13), then,

$$e^{-\frac{x_i}{\tau_i}} = \frac{\lambda \tau_i}{r_i} \quad (\text{III-14})$$

$$x_i = \tau_i \ln \frac{r_i}{\lambda \tau_i} \quad (\text{III-15})$$

¹I. S. Sokolnikoff and E. S. Sokolnikoff, Higher Mathematics for Engineers and Physicists (New York: McGraw-Hill Book Company, Inc., 1941), pp. 163-67.

Now, as was provided in equation (III-11), the only solutions to be admitted are those for which

$$x_i \geq 0.$$

This in turn requires that the argument of the logarithm in (III-15) be ≥ 1 . These constraints can be re-stated by acknowledging that for any i , either

$$x_i = 0 \quad \text{(III-16.1)}$$

$$\text{or} \quad \lambda \leq \frac{r_i}{r_i} \quad \text{(III-16.2)}$$

To explain the significance of equation (III-16), assume that of a total of n possible projects, some sub-set, σ , of the n , constitutes those projects which will be funded for optimum return. For each of the projects in σ , since x_i is non-zero, then

$$\lambda \leq \frac{r_i}{r_i}.$$

Of course, for all projects not in σ

$$x_i = 0.$$

In view of this, equation (III-11) becomes

$$B = \sum_{\sigma} x_i. \quad \text{(III-17)}$$

Now, combining equation (III-17) with equation (III-15), one obtains

$$B = \sum_{\sigma} \tau_i \ln \frac{r_i}{\lambda \tau_i} \quad (\text{III-18})$$

which can be re-written as

$$\lambda = \exp \left\{ \frac{\sum_{\sigma} \tau_i \ln \frac{r_i}{\tau_i} - B}{\sum_{\sigma} \tau_i} \right\} \quad (\text{III-19})$$

It is evident that the value of λ is a function of the projects selected for inclusion in σ . Consequently, in order to implement this optimization procedure, some approach is needed which avoids the necessity of considering the multitude of possible combinations for σ .

To reduce the effort required in seeking σ , the following algorithm can be employed. First, the candidate projects must be arranged in descending order of the cost-benefit factor,

$$\frac{r_i}{\tau_i} .$$

In other words, arranged such that

$$\frac{r_1}{\tau_1} > \frac{r_2}{\tau_2} > \dots > \frac{r_n}{\tau_n} . \quad (\text{III-20})$$

Next, σ is assumed to consist of project 1 and project 2, and λ is computed from equation (III-19). Then, equation (III-16.2) is invoked for $i = 1$ and $i = 2$. If equation (III-16.2) is satisfied in each case, then σ is expanded to include project 3. The subset, σ , is

progressively expanded, with λ being re-computed and checked at each iteration. The iterations cease when a project, k , is introduced into the subset such that equation (III-16.2) is violated. Having reached this point, it is determined that σ consists of the first $k-1$ projects in the ordering of equation (III-20). Then, λ is computed from equation (III-19), including the selected $k-1$ projects. Finally, for this value of λ , the fund allocation for each of the selected projects, x_i , is determined from equation (III-15). This algorithm is shown, in flow-chart form, in Figure 11.

Should σ consist only of project 1, the combination of equations (III-15) and III-19) reveals that

$$x_1 = B ,$$

as one would expect. If, then, project 2 is added to σ , λ increases as per equation (III-19). Likewise, x_1 is decreased to some value less than B , with the balance being allocated to x_2 :

$$x_1 < B$$

$$x_2 = B - x_1 .$$

As λ increases with each addition of a project to σ , it is compared, via equation (III-16.2), with successively smaller values of

$$\frac{r_i}{\bar{r}_1} .$$

FIGURE 11
SELECTION ALGORITHM

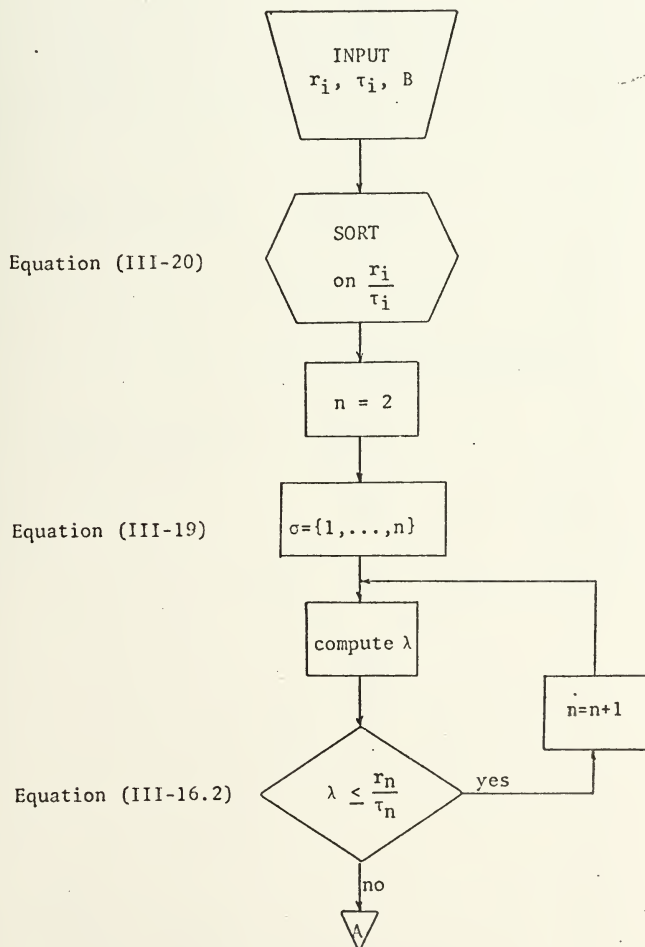
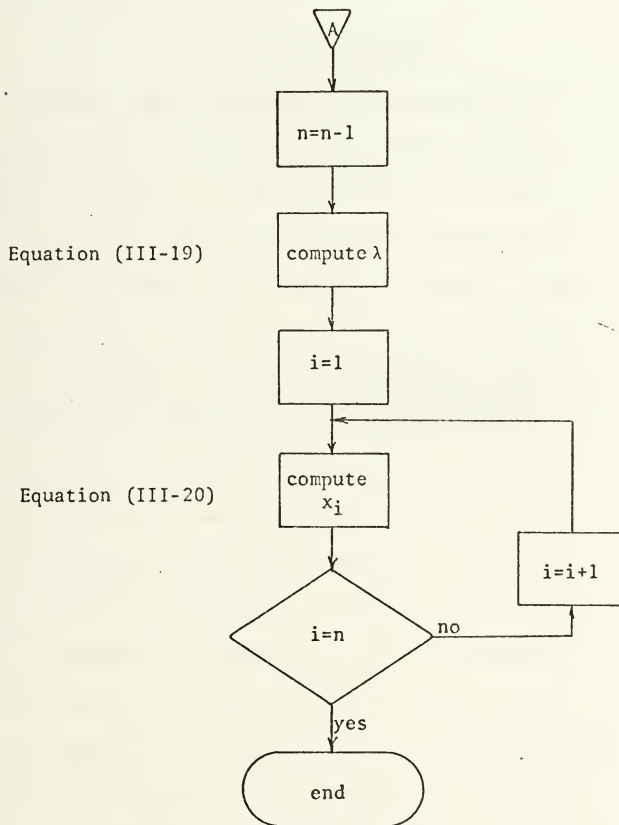


FIGURE 11--Continued

Source: Original

Consequently, if one were to plot the values of λ and the limiting $\frac{r_i}{r_1}$ for the series of σ 's, the resulting locus as illustrated in Figure 12, would demonstrate the determination of σ .

Project Control

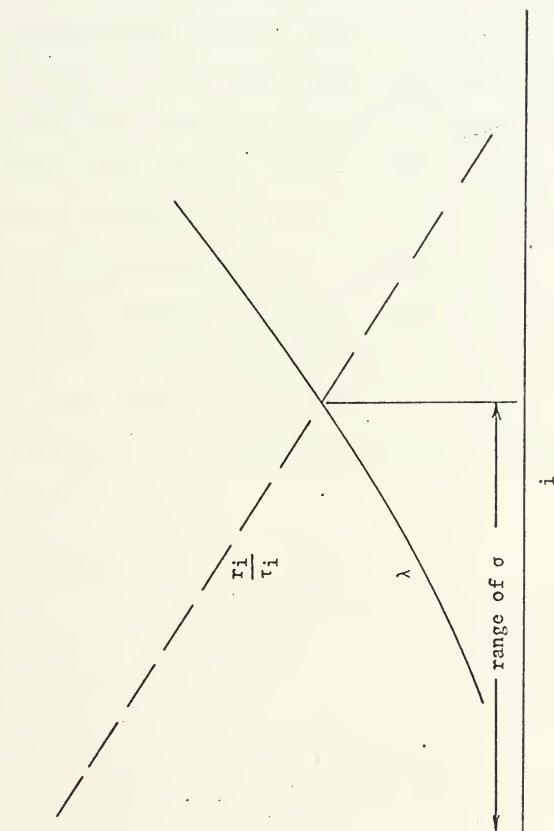
Chapters I and II examined various aspects of management planning and control as they relate to the administration of research and development activities. As was illustrated in Figure 1, the essential ingredients of the control process are the comparison of actual and anticipated performance and an analysis of observed variances. Though this description of control is, at face value, no different from control in conventional management systems, the significance of control in research and development activities, in terms of its importance for effective and efficient operations, is substantially greater.¹ As has been mentioned before, control in most enterprises relates to budgetary control of operations. Traditionally, this type of control has been successful. The degree of success that has been realized is largely attributable to the close correlation between resources expended and results realized. Such a nexus is obvious in practices which include instruments like standard cost schedules for production activities. Even less structured business operations, such as advertising, are amenable to analysis on the basis of results realized from given expenditures.²

¹Souder, "Experiences with an R & D Project Control Model," p.39

²Jay W. Forrester, Industrial Dynamics (New York: The M. I. T. Press of the Massachusetts Institute of Technology and John Wiley & Sons, Inc., 1961), pp. 199-207.

FIGURE 12

CONVERGENCE OF $\frac{\tau}{\tau}$ AND λ TO
DETERMINE OPTIMUM SUBSET, σ



Source: Original

As was discussed in Chapter II, personnel associated with research and development activities have, in several studies, demonstrated an ability to estimate anticipated expenses. However, other similar attempts have resulted in notable failure. The essential characteristic of research and development which prompts this dichotomy is the uncertain correlation between the activities for which cost estimates are prepared and the research advances which are expected to result from these activities. The supervisor of a research team may be quite capable of estimating costs for a proposed project. However, the accuracy of his estimates, regards the effectiveness of his organization in persuing and achieving technological advance, may be inadequate.¹ As a result, control, in research and development, must monitor not only expenditures, but, also, the achievements accruing from these expenditures.

The Role of Prior Estimates

A prior estimate is a statement of anticipated circumstances associated with some future effort. The fact that prior estimates concern future efforts is essential, since the estimate is presumed to exclude any information of the actual prosecution of the effort. In this respect, then, an annual budget is a prior estimate since it concerns activities which are yet to commence.

Prior estimates are common to the vast majority of managerial endeavors. Besides financial budgets, such estimates include production

¹Jantsch, Technological Forecasting In Perspective, pp. 53-60.

schedules, personnel projections, material schedules, and the like. These estimates derive from objectives which are the results of planning. Most such estimates, for conventional commercial concerns, are fairly objective from the stand point that the estimates relate to activities of very similar nature. Therefore, these estimates are frequently obtained by using simple, extrapolation techniques.

Estimates for research and development activities are also obtained, in most instances, by extrapolation techniques. Indeed, one of the most common techniques employed in forecasting, or estimating, is that of trend extrapolation.¹ The use of these common extrapolation techniques presumes the existence of a substantial amount of continuity between the past and the immediate future. As has been emphasized in preceding discussion, such a presumption, relating to research and development, is inherently uncertain. Specifically, two questions must be addressed when extrapolation is attempted:

1) What are the criteria for choosing curves to extrapolate?

2) When is it safe to extrapolate a rate of change naively, and when must points of inflexion, or changes in the rate of change, be anticipated?²

These are two questions that are not easily answered.

In attempting to minimize the impact of poor results from

¹James R. Bright, Technological Forecasting for Industry and Government--Methods and Applications (Englewood Cliffs: Prentice-Hall, Inc., 1968), pp. 56-109.

²Ibid., p. 77.

extrapolation methods, many management systems, for research and development, implement measures of the uncertainty involved, in the form of probabilities of success. However, these estimates are still prior estimates, and are usually obtained by some sort of extrapolation. The difficulties associated with such efforts have been discussed in Chapter I.

The over-riding consideration that must be recognized when addressing the topic of prior estimates, is that the utility of such techniques, as they have been applied to conventional management systems, is limited. Even attempted modifications, which have been initiated as a result of this limited utility, are difficult to implement, and frequently have the same inherent shortcomings possessed by the methods which they seek to improve.

From the preceding analysis, one should not conclude that prior estimates have no valid role in research and development management. On the contrary, they are essential. All quantitative selection techniques initiate from prior estimates. It should be obvious, that, since selection necessarily takes place before any activity commences, only prior estimates are available. Project control, however, is a continuing process throughout the life of the project. It is reasonable, therefore, to contend that one need not and should not rely solely on prior estimates for purposes of control. The remainder of this chapter will be devoted to the development of a method which augments prior estimates and affords increased vitality for methods of controlling research and development activities.

Description of the Process

The preparation of a proposal for a research project necessarily commences with the development of a scenario. This scenario is a time-ordered, episodic sequence of events which are logically related to one another and portray the intended progress of the project. In informal organizations, scenario preparation may be nothing more than the act of mentally planning the effort. However, in most situations, which require the submittal of proposals in a formal fashion, the scenario will be explicit in nature and set forth in a published form. Procedures for the preparation of proposals, and attendant scenarios, vary greatly among research organizations. The particular approach described here is one common to some of the laboratories of the U.S. Navy.¹

To illustrate the development of a scenario, it is advantageous to examine a sample project. For this purpose, consider the task of developing a technique for the optimal design of super-conducting electrical machines, intended for solution by electronic computer. The reason this task would be considered a research and development effort derives from the fact that super-conducting machines do not conform to conventional electro-magnetic theory and established design procedures have not been developed. Moreover, since past design and construction endeavors have been limited to a few prototypes, there exists no consensus concerning an accepted theoretical model.

¹The method discussed is one obtained during the period of time when the author was Program Officer at the Naval Applied Science Laboratory, Brooklyn, New York.

A scenario for this research task consists of the following events:¹

- 1) Determine appropriate parameters for the model.
- 2) Develop magnetic field equations utilizing the parameters selected.
- 3) Determine operational and theoretical constraints.
- 4) Develop design equations.
- 5) Develop optimization method.
- 6) Write machine program.
- 7) Complete analysis of test designs.

The initial opinion one is likely to formulate, concerning such a scenario, is that it is simply an application of conventional scheduling techniques, such as PERT.² To an extent, such an observation is correct, for both serve to indicate project progress, or schedule. However, it should be observed that the scenario is a sequential ordering which does not include any activities conducted concurrently. On the contrary, the events in a scenario can be arranged only in serial order. Consequently, completion of a scenario event indicates explicitly some degree of project progress. Nonetheless, PERT network analysis is of sufficient similarity to the scenario to deserve further examination. The results of such an examination has

¹D. P. Greeneisen, "A Design Program for Superconducting Electrical Machines" (unpublished M. S. thesis, Massachusetts Institute of Technology, 1968), pp. 7-21.

²C. McMillan and R. F. Gonzalez, Systems Analysis--A Computer Approach to Decision Models (Homewood, Illinois: Richard D. Irwin, Inc., 1968), p. 292.

revealed that PERT techniques can be used in more complex projects where a single scenario may be difficult to identify.¹ However, PERT analysis will be useful only in those instances where projects can be sub-divided into appropriately isolated subsidiary networks which meet certain criteria.² In the final analysis, therefore, whether one considers the scenario to be a special case of the general PERT technique is relatively immaterial. The only essential characteristics, which must be maintained, are those of sequentiality in the ordering of events, and the selection of events such that technological progress is represented. The importance of the second requirement will become obvious as the description proceeds.

After determining the events of the scenario, described above, it is necessary to prescribe a procedure for utilizing the data contained therein. Towards this end, it is beneficial to use the scenario events as milestones for the computation of time and cost projections.

The development of a time projection may come about in one of two possible ways. First, time until completion may be a given constraint on the problem. In this case, it is necessary to determine a manning level distribution which will be adequate to meet the time requirement. The second possibility is that personnel resources may be a governing constraint. This, in turn, would require an estimate of

¹Rosenbloom, "Notes on the Development of Network Models for Resource Allocation R & D Projects," p. 62.

²R. E. Beckwith, "A Cost Control Extension of the PERT System," IRE Transactions on Engineering Management, EM-9 (December, 1962), p. 147.

the time needed for completion with the given personnel available. In actuality, a third possibility exists, for which neither personnel available nor time is specified. In this instance, a trade-off between the two would be sought. The solution for the optimal trade-off is a substantial task in itself, and will not be pursued further.¹

Regardless of which one of the two situations occurs, the determination of manpower or time requirements, from the given constraint, entails an estimate of personnel capability in terms of the particular project under consideration. Any research organization contains a great variety of skills and many levels of efficiency. Certain personnel may have had experience which would be particularly applicable to the project under consideration. If these personnel are available, completion of the project will be expedited. If, on the other hand, some novice talent is to be used, added time for completion must be allowed. The primary consideration which is being addressed, here, is that time estimates are functions of the administrator's expectations regards the technological demands of the research project, and his estimates of the capability of a given research team to cope with these demands.

In addition to the preparation of a time estimate, the scenario will form the foundation of a cost estimate. Ideally, the scenario will be composed of events which represent stages of the project which are reasonably different in terms of the activity involved. For

¹For further consideration of this question see R. A. Goodman, "Organization and Manpower Utilization in Research and Development," IEEE Transactions on Engineering Management, EM-15 (December, 1968), p. 198.

instance, in the example scenario which has been presented, the determination of operational and theoretical constraints and the development of design equations are related to the extent that one is required for the accomplishment of the other, and both are dealing with the same subject matter. However, the actual activity involved in each, in terms of solution methods, reference material and theoretical considerations, will be different. This independence of scenario events is not a theoretical imposition, but, simply serves to facilitate time and cost analyses.

Cost estimates for scenario events are obtained from data concerning the number and grade of personnel employed, the duration of employment, necessary expenditures for equipment and facilities and appropriate allowances for overhead assignments. This consideration of the elements of cost is in keeping with conventional budget preparation practices. What is different is the relatively tenuous accuracy of the cost estimate. If the administrator, in the preparation of the estimate, has improperly assessed any one of several technological aspects of the project, the effort anticipated in the funding estimate may be entirely inappropriate or inadequate for the task.

Using the events of the scenario as milestones of progress, it is possible to develop cost-achievement and time-achievement functions for the project. Before these functions can be expressed, a measure of achievement must be determined. Methods of indicating achievement are numerous. One such method, in terms of \dot{a}_{xi}/a_M , was discussed earlier in this chapter. Due to the diversity of activity

that is experienced during the course of a project, however, a single parameter may not be appropriate. A more useful measure of achievement for measuring project progress can be obtained as follows:¹

Each event in the scenario is assigned a weight, w_i , representing the degree of difficulty associated with the event, measured on a 0 to 10 scale. These weights are then multiplied by the number of man-hours assigned to corresponding events, h_i . The amount of achievement for a given event, a_i , is then equal to its weighted number of man-hours divided by the total weighted man-hours. That is,

$$a_i = \frac{w_i h_i}{\sum_i w_i h_i}$$

With achievement thus determined, the required functions can then be plotted as in Figure 13.

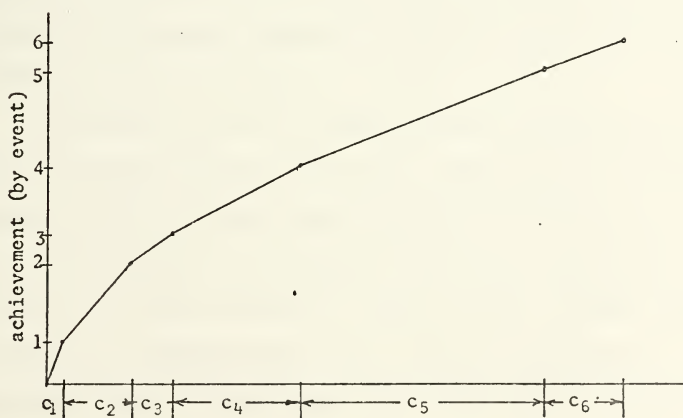
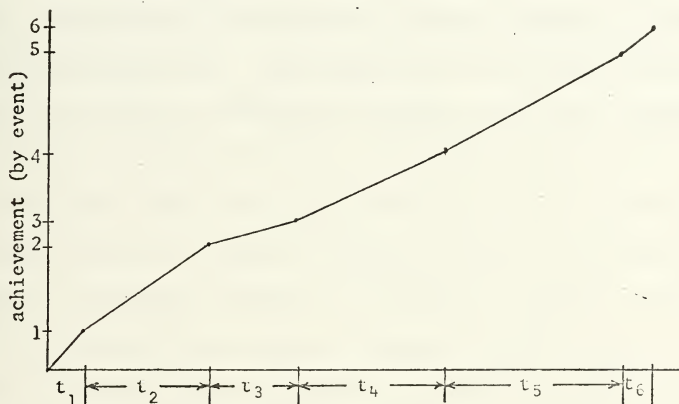
The Theoretical Model

Having obtained time-achievement and cost-achievement relationships, the next step is to determine how they may be used in project control. If such a determination is to be made, it will be necessary to obtain an analytical model for the research and development process.

The development of a model can commence with an analysis of the time function. To begin, it is recognized that the time for completion of any event is a random variable. The estimated value of time, \bar{t}_i , will rarely be the actual time realized. Whether \bar{t}_i is greater or

¹Souder, "Experiences with an R & D Project Control Model,"

FIGURE 13
PROJECT TIME AND COST FUNCTIONS



Source: Original

smaller than the actual t_i will depend on the ability of the person making the estimate to anticipate a number of unknown factors in the project. These factors include the suitability of the research team to the task, correct assessment of technological difficulties, and so forth. To account for the uncertainty involved, consider a hypothetical *man-day of work*. This quantity is a Tayloristic type of measure and relates to the amount of progress that can be realized by a man who is ideally suited for a particular task and devotes 100 per cent of his time to that task for one working day. Assuming, also, that each portion of a project, leading to an event, is composed of relatively homogeneous activity, it can then be assumed that each event is composed of some number of man-days of work. Moreover, this number of man-days of work is intrinsic to the event itself, given the state of the project from the preceding event. With this concept in mind, one realizes that \tilde{t}_i is an estimate of some t_i^* , the number of calendar days actually required to accomplish the number of man-days of work associated with event i .

The quantity t_i can be examined further by letting k be the number of man-days of work for the event. Then, t_i is the number of calendar days that will pass before k man-days of work are realized. Since project progress is not constant, the number of calendar days for any given man-day of work is a random variable. Assume, then, that there is a probability of p that a man-day of work will be accomplished in the calendar interval Δt . From this, one observes

that the variable t_i is Poisson distributed.¹ That is,

$$p(t_i; \mu) = \frac{\mu^{t_i} e^{-\mu}}{t_i!} \quad (\text{III-21})$$

where μ is the mean of t_i .

The Poisson model thus derived can be related to the real world by examining the meaning of the parameters involved. For instance, k , the number of man-days of work, will be a function of the discipline involved (for example high-energy physics or basic mechanics) and the degree of achievement to be realized as stated in the project objectives. The probability, p , and mean, μ , will be functions of the expertise of the research group and the effectiveness of supervision.² In retrospect, then, \tilde{t}_1 is an estimate of a random variable, t_1 . Errors in the estimate; or $t_1 - \tilde{t}_1^*$, may be due to two factors. One factor is the random nature of t_1 . The estimate, at best, is an estimate of the mean, μ . Another factor is a lack of competence or the presence of bias on the part of the person making the estimate.

After determining a model for the time parameter in the research and development process, it is not difficult to adopt it to the cost factor. Recognizing that direct costs are a strong function of project time, one can see that each man-day of work will relate to an intrinsic cost. Thus, the cost of a project will also be Poisson

¹Richard C. Dubes, The Theory of Applied Probability (Englewood Cliffs, New Jersey: Prentice-Hall, Inc., 1968), pp. 168-70.

²The use of a Poisson model was suggested by Asher, "A Linear Programming Model for the Allocation of R & D Efforts," p. 156.

distributed and can be expressed by

$$p(c_i; \mu) = \frac{\mu^{c_i} e^{-\mu}}{c_i!} \quad (\text{III-22})$$

The factors affecting the accuracy of cost estimates are the same as those that applied to the time parameter.

Analysis and Inference

The ultimate objective of the preceding analysis is the realization of a method for improving project control techniques. As has been discussed at some length, most control techniques are based on prior estimates. These estimates, however, relate to processes that are inherently uncertain as to outcome. What is needed, then, is a method for measuring the accuracy of prior estimates as they relate to the actual situation.

The purpose of developing a scenario is to obtain time and cost estimates for a project. In doing this, several intermediate estimates, for scenario events, are made. In considering these prior estimates, the manager is interested in knowing whether the estimates for total time,

$$\bar{T}_i = \sum_i \bar{t}_i \quad (\text{III-23})$$

and total cost,

$$\bar{C}_i = \sum_i \bar{c}_i \quad (\text{III-24})$$

are accurate within certain bounds. Most control methods delay conclusions, regarding estimate accuracy, until the final stages of a project. However, if the mechanism of the time and cost processes are understood, such conclusions may be anticipated.

The errors in prior estimates, that the manager needs to identify, are those resulting from incompetence or bias on the part of the person making the estimate. If this person is of a conservative nature, he may arbitrarily introduce some amount of margin in all estimates to protect himself from unanticipated problems. Or, if he is a perpetual optimist, he may always underestimate cost, or overestimate the ability of the research team. If these errors in the total estimate do exist (the fact that is to be determined) it is reasonable to expect that proportionate errors will exist in each of the estimates for the events composing the total. Therefore, as the first few events are reached, the time and costs actually experienced should be useful in anticipating the ultimate outcome. Some techniques which are available for the use of data, obtained subsequent to the prior estimate, can be illustrated by considering the following example: Let the data shown in Table 7 be the prior estimates of cost, for a project consisting of seven scenario events, and the actual costs realized for the first four events. For the moment though, assume that only event 1 has been completed.

After the occurrence of event 1, the data in Table 7 is related as

$$\bar{c}_1 = 10$$

and $c_1^* = 14.0$.

TABLE 7
PROJECT COST DATA

Event	Estimate	Actual
1	10.0	14.0
2	14.0	18.5
3	8.0	12.3
4	24.0	32.0
5	31.0	--
6	12.0	--
7	9.0	--

The objective of a control process, at this point, is to determine whether the error

$$c_1^* - \tilde{c}_1 = 4.0 \quad (\text{III-25})$$

is significant. The pertinent facts which will be used, in determining significance, are that the random variable, c_1 , is Poisson distributed, and that \tilde{c}_1 is the estimate of the mean, μ . The appropriate hypothesis in this instance is

$$H_0 : c_1 = \mu \leq 10 .$$

Assuming H_0 , the test to be conducted is to determine the probability that the actual results, c_1^* , would occur if

$$\mu = \tilde{c}_1,$$

which is¹

$$\sum_{c_1=14.0}^{\infty} p(c_1;10) = 1 - \sum_{c_1=0}^{14.0} p(c_1;10) \quad (\text{III-27})$$

This test is accomplished with the use of the cumulative Poisson distribution which is tabulated in Appendix B. Using Equation (III-26), one finds

$$\begin{aligned} \sum_{c_1=14.0}^{\infty} p(c_1;10) &= 1 - P(14.0;10) \\ &= 1 - 0.917 \\ &= 0.083 \end{aligned}$$

If a confidence limit of

$$\alpha = .05$$

had been adopted, then, since

¹Ya-lun Chou, Statistical Analysis (New York: Holt, Rinehart and Winston, Inc., 1969), p. 209.

$$.083 > \alpha$$

the hypothesis, H_0 , would be accepted. This amounts to accepting the variation of Equation (III-25) as being insignificant due to the random nature of the variable c_1 .

As more data becomes available with accomplishment of other events, as shown in Table 7, a more comprehensive analysis is possible. The type of analysis most commonly employed, with several samples from similar populations, is the χ^2 test.¹ This test is especially useful in testing Poisson distributed data. Application of the χ^2 test for the data in Table 7 is illustrated in Table 8.

As is indicated in Table 8, χ^2 is a measure of the sum of the squares of the variates, as calculated by

$$\chi^2 = \sum_i \frac{(c_i^* - \bar{c}_i)^2}{c_i} \quad \text{(III-28)}$$

In order to develop a χ^2 analysis, it is necessary to determine the degrees of freedom of the samples. Without pursuing a lengthy discussion of the meaning of this quantity, suffice it to say, that, in the type of analysis being conducted, when n samples are available, there will be n degrees of freedom.²

¹C. G. Paradine and B. H. P. Rivett, Statistical Methods for Technologists (London: The English Universities Press, Ltd., 1960) p. 79.

²Ibid., pp. 76-77.

TABLE 8
 χ^2 ANALYSIS OF DATA

event	observed value, c^*	estimated value, \bar{c}	$(c^* - \bar{c})$	$\frac{(c^* - \bar{c})^2}{\bar{c}}$
1	14.0	10.0	4.0	1.60
2	18.5	14.0	4.5	1.45
3	12.2	8.0	4.2	2.21
4	32.0	24.0	8.0	2.66
				$\Sigma = 7.92 = \chi^2$

Source: Illustrative data

The results of a χ^2 test are obtained from a comparison of a computed value of χ^2 with a theoretical value determined on the basis of the degrees of freedom and the confidence limit, α . Referring to the tabulation in Appendix C, it is seen that, for four degrees of freedom ($\nu=4$) and $\alpha=.05$, then the theoretical value of χ^2 is 9.49. Then, since

$$\sum_1 \frac{(c_i^* - \bar{c}_i)}{c_i} = 7.92 \leq 9.49 \quad (\text{III-29})$$

one would conclude that the variations, $c_i^* - \bar{c}_i$, are within the limits of error attributed to the random nature of c_i .

Control Aspects of the Analysis

Both of the statistical tests, conducted in the preceding section, resulted in the conclusion that observed errors between estimated and actual expenditures were attributable to random variation. However, observing the data in Table 7, one is struck by the fact that the test results were obtained from data which demonstrated cost overruns of up to 50 per cent. It is natural, then, to question the usefulness of these methods. However, certain factors in the analysis should be examined.

First, it is necessary to recognize the statistical character of the Poisson distribution. For instance, the distribution has a pronounced skewness that is a function of the value of the random variable. In addition to this, the variance is equal to the mean, which

generally results in substantial dispersion. The end result of these characteristics is that statistical tests based on Poisson data are often less precise than tests of normally distributed data, for example. From this, one can conclude that conventional confidence limits (for example $\alpha=.05$ or $\alpha=.01$), which are generally associated with normal data, may be insufficient constraints for tests of Poisson data. For example, the selection of $\alpha=.10$ vice $.05$ would have changed the conclusions drawn from the example tests.

A second important consideration, which must be recognized, relates to the model which was developed. It must be recalled that the data and tests which were conducted were drawn from a model which correlated, for instance, cost and technological achievement. It must further be recognized that overruns in research expenditures frequently far exceed the 50 per cent level. At this point, one might wonder about research and development projects which, after demonstrating excessive costs in early stages, are constrained to complete within final budget figures. This does frequently occur. However, what invariably accompanies such forced adherence to budget limitations is a concomitant revision of project objectives. Consequently, though expenditure estimates may ultimately be proven to be quite accurate, considerable shortcomings will probably be realized in estimates of achievement.

These considerations certainly are significant in any discussion of the usefulness of the statistical techniques that have been developed. It is fairly obvious that two conditions must be satisfied if such inferential analysis is to be useful. Any organization which

might consider such techniques must first of all identify a realistic confidence limit, in keeping with what are considered to be acceptable amounts of over-run. Second, it is necessary to ensure that achievement parameters associated with project events are valid measures of technological advance. If this is assured, it must then be accepted that attempts to enforce budgetary requirements will likely result in a failure to achieve intended project objectives.

Statistical techniques, such as those examined, do offer additional methods for controlling research and development. By conducting periodic review which incorporates these techniques, additional information can be obtained which is pertinent to project control. However, action resulting from such review, whether it be to continue, terminate, re-evaluate or whatever, must be based on an understanding of what is actually indicated by the results of the analysis.

Summary

Chapter II was devoted to the description of numerous methods which have been used or have application in project selection and control. This chapter has addressed the problem of selecting particular techniques, from among those available, which are considered especially useful in the selection process. In addition, a statistical control technique, not found elsewhere in the literature, has been developed, and its applicability in project control has been discussed. Though the several ingredients, of the selection and control techniques that have been developed, have been drawn from other sources, it is considered that the organization of these ingredients that has been given in this analysis constitutes a new approach to project selection and

control. The procedure for implementing the selection technique has been adequately summarized in Figure 11. This technique has emphasized the use of information that is generally available in research and development administrative efforts. It has minimized the use of data which might require special collection efforts. In particular, it has avoided the use of estimates of probabilities of success.

The development of the statistical technique for use in project control has addressed only the basic theoretical concept which forms its foundation and the demonstration of its use. Actual application of the technique has not been pursued because of the variety of methods in which it might be used. As has been emphasized, the frequent exclusive reliance on prior estimates for project control is fraught with uncertainty. Yet, prior estimates are an essential part of any control system. The statistical technique developed herein is offered as an adjunct to existing control methods. It is considered that this technique, when combined with usual management control practices, will improve the control of research and development programs. This technique should enhance management by exception in that it offers more objective data for use. This technique is deemed to be particularly desirable because of the manner in which project achievement is necessarily incorporated in the analysis. Presumably, this will serve to minimize the need for presumptions regarding progress that are usually drawn from an examination of time and cost accounting data.

The theoretical treatment that has been offered in this chapter is noticeably not rigorous. The reason for this is that the component methods of the techniques that have been developed were drawn

from other sources and are presumed proven. . Emphasis has therefore been placed on the organization of these components into a useful, coherent system.

CHAPTER IV

CONCLUSIONS

This paper has attempted to present an analysis of research and development practices with the intent of producing techniques which should be useful in efforts associated with project selection and control. This effort was prompted by notable shortcomings in existing management methods as evidenced by widely publicized failures in major programs. As has been discussed in the preceding chapters, several problem areas can be identified as being likely candidates for the cause of the difficulties that have been experienced. Foremost among these is the inadequacy of data employed in present systems.

Most contemporary efforts in the management of research and development rely on the use of data adopted from established production-type operations. These data, invariably, emphasize the use of time and cost accounting. However, as has been forcibly asserted throughout the preceding, this reliance on conventional methods is inadequate; due to the nature of research and development activities. One should be able to conclude, by now, that controlling by budget overruns and underruns only, can be seriously misleading. It must be emphasized that it is the interaction of both cost and achievement that determines the actual status of a research project. The conclusion that must be drawn from this is that existing methods of controlling projects on the basis of budgetary variances, which are time and cost functions

alone, are exercises in futility and, at best, will only give the manager a deceptive feeling of satisfaction concerning the conduct of a project. This deception is realized only in the latter stages of a project when it is necessary to accept the fact that objectives will not be accomplished without additional expenditures. It is at this point, when most of the available funds have already been spent, that the manager feels compelled to spend more in order to avoid criticism for having apparently wasted preceding expenditures. The only method available to alleviate such an "eleventh-hour" panic is to continually measure progress throughout the life of the project. The effectiveness of such monitoring will, of course, depend on the efficiency of any analyses of variances which do occur.

In an attempt to account for the factors of uncertainty which are frequently encountered, some techniques have relied on a measure of the probability of success of subject projects. It should be patently obvious that reliance on such a device pre-empts any efforts to monitor subsequent performance. The probability of success measure per force assumes that any determination regards success must be reserved until the conclusion of a project. By adopting this technique, the manager neglects his responsibility to assess, in a continuing fashion, the successful progress of a project.

These shortcomings would necessarily lead one to conclude that existing methods for the management of research and development are not sufficient for their intended purposes. The reasons that more failures than those experienced have not occurred are that management has been quite fortunate or research objectives have been sacrificed to

avoid overruns.

The techniques for project selection and control that have been presented in this paper, specifically in Chapter III, are regarded as offering at least a partial solution for the difficulties that have been discussed. The selection technique demands little in terms of the effort required for implementation. The theory is straight-forward and the assumptions are obvious. The very simplicity of the method will likely invite criticism that such simplicity cannot possibly account for the numerous intricacies of research and development projects. Such criticism cannot be denied. Indeed, it has been a major premise of this paper that such intricacies are of paramount importance and cannot be accounted for in any selection process, regardless of the degree of sophistication that might be offered. However, it must be acknowledged that the proposed selection technique is intended for use with the control technique which was the second subject of the preceding chapter.

It has been stressed that any selection method must be based on prior estimates. However, efforts to refine such methods of selection cannot achieve a degree of certainty beyond the limits imposed by the inherent nature of the projects being selected. Therefore, instead of continuing to emphasize the importance of selection, one must begin to develop methods to monitor progress so that selection errors can be identified in a timely fashion. Selection, then, must be regarded as only a portion of a system for project management. The other very important portion of the system is, of course, control.

The development and implementation of an effective control

technique, relieves, somewhat, the burden placed on selection. If effective, control will identify errors that have been made in selection so that funds can be diverted to more successful endeavors. Thus, the development of an effective control system is possibly the best means of accounting for uncertainty in research and development.

A final aspect of statistical control, that deserves mention, is that of credibility. As was shown in the preceding chapter, it may, at times be difficult to accept the results of analysis. This is, however, a difficulty that is fairly common among Bayesian-style methods. It can be said, in this respect, that such techniques are useful only when implemented in an adequate fashion. Attempts to institute such systems on a partial or "trial" basis seldom prove satisfactory since subjective influences invariably offset the prescriptions of objective techniques.

These prospects of difficulties in implementation prompt consideration of certain aspects of the proposed techniques which deserve further examination and refinement. The first of these is the need to develop a uniformly most powerful test for the Poisson model. The tests proposed in this paper are valid for any situation; however, in some circumstances they may be found lacking in precision. It would therefore be advantageous to find a method for structuring data such that the tests will yield consistent results for any circumstances.

A second consideration of the model which merits further development is the need to ensure accurate portrayal of technological progress in project scenarios. As has been discussed in preceding sections, the measurement of progress in research activities is often

quite difficult. However, for the techniques proposed it is absolutely essential that events be selected such that progress is explicitly indicated. Procedures for identifying adequate indicators and computing progress need further development in order that this requirement can be satisfied.

A final aspect which needs more refinement concerns the optimization method that has been selected. To be specific, this method does not allow for minimal funding of projects. In many instances, though a project may not be included in an optimal funding program, management may wish to provide some token appropriation in order that project continuity is not lost completely. For the proposed technique, though, a project is either funded at an adequate level or not at all. It therefore would be worthwhile to formulate a selection technique which will optimize allocations with the provision that a certain number of non-optimum projects may receive some funding.

APPENDIX A

MISSION-TECHNOLOGY VALUE MATRIX¹

An essential part of the selection process is the identification of the contribution of technologies to known objectives. These matrices were developed for the purpose of relating technologies, supported by Naval Research efforts, to the research objective areas of the Naval Materiel Command, Washington, D.C. These matrices are included to illustrate the degree of detail required in identifying all pertinent relationships. Counterparts to these matrices would be similar ones which relate sciences to the technologies given.

¹Marvin J. Cetron, "QUEST Status Report," IEEE Transactions on Engineering Management, EM-14 (March, 1967), p. 58.

TECHNOLOGY

INTELLIGENCE & SURV. TECHNOLOGY

- Intelligence Tech.
- Ground-based Surv. Tech.
- Recon. & Airborne Surv. Tech.
- Undersea Surv. Tech.
- Multipurpose Surv. Tech.
- Electromagnetic Warfare

FIREPOWER TECHNOLOGIES

- Nuclear Weapons' Tech.
- Conventional Munitions Tech.
- Naval Non-guided Weap. Tech.
- Chemical-Biological Weap. Tech.
- Guided Miss. & Weap. Guidance
- Damage Detect. & Scoring Tech.
- Aerospace Vehicle Defense

MULTIPURPOSE TECHNOLOGIES

Materials
Environment
Human Performance
Electronic Multipurpose Tech.
DEMAND AND CONTROL TECHNOLOGIES
Communications Tech.
Control Tech.

MISSION

STRIKE WARFARE

Airborne Attack
Surface Attack
Submarine Attack
Amphib. Assault
Seabased Strategic
Deterrence
Airborne Anti-Air
Warfare
Surface Anti-Air
Warfare

ANTI-SUB WARFARE

Airborne ASW
Surface ASW
Submarine ASW
Undersea Surv.
Mining
Mine Cntr-meas.
ASW Support

COMBAND SUPPORT

Command & Control
Naval Communicat.
Electr. Warfare
Navigation
Ocean Surv.
Recon. & Intell.
Environmental
Systems
Special Warfare

TECHNOLOGY

MOBILITY TECHNOLOGIES

Turbine & Ramjet Engine Tech.
 Rocket Propulsion Tech.
 Surface Mobility Component Tech.
 Electric Propulsion Tech.
 Power Generation Tech.
 Fuels, Lubricants and Hazards
 Structures
 Flight Mechanics
 Flight Control
 Aircraft Tech.
 Vehicle Dynamics
 Vehicle Environmental Control
 Vehicle Mechanical Subsystems
 Recovery and Crew Station
 Vehicle Life Support
 Space Support Tech.
 Navigator Tech.
 Surface Mobility Studies

LOGISTICS TECHNOLOGIES

Propulsion Ground Support Tech.
 Surface Nuclear Power Appl.
 Basing Tech.
 Biocastronics for Pers. Selec.
 Medicine and Surgery Tech.
 Food Tech.
 Material Handling Tech.
 POL Tech.
 Logistics Vehicles
 Maintenance and Support Tech.

APPENDIX B

CUMULATIVE POISSON DISTRIBUTION¹

A tabulation of

$$F(x) = P(c \leq x) = \sum_{c=0}^x e^{-\lambda} \lambda^c / c! .$$

¹Bernard Ostle, Statistics in Research (Ames, Iowa: The Iowa State University Press, 1963), pp. 372-76.

$\lambda \backslash x$	0	1	2	3	4	5	6	7	8	9	10
0.01	0.990										
0.02	0.980										
0.03	0.970										
0.04	0.961	0.999									
0.05	0.951	0.999									
0.06	0.942	0.998									
0.07	0.932	0.998									
0.08	0.923	0.997									
0.09	0.914	0.996									
0.10	0.905	0.995									
0.15	0.861	0.990	0.999								
0.20	0.819	0.982	0.999								
0.25	0.779	0.974	0.998								
0.30	0.741	0.963	0.996								
0.35	0.705	0.951	0.994								
0.40	0.670	0.938	0.992	0.999							
0.45	0.638	0.925	0.989	0.999							
0.50	0.607	0.910	0.986	0.998							
0.55	0.577	0.894	0.982	0.998							
0.60	0.549	0.878	0.977	0.997							
0.65	0.522	0.861	0.972	0.996	0.999						
0.70	0.497	0.844	0.966	0.994	0.999						
0.75	0.472	0.827	0.959	0.993	0.999						
0.80	0.449	0.809	0.953	0.991	0.999						
0.85	0.427	0.791	0.945	0.989	0.998						
0.90	0.407	0.772	0.937	0.987	0.998						

$\lambda \backslash x$	0	1	2	3	4	5	6	7	8	9	10	11
0.95	0.387	0.754	0.929	0.984	0.997							
1.00	0.368	0.736	0.920	0.981	0.996	0.999						
1.1	0.333	0.699	0.900	0.974	0.995	0.999						
1.2	0.301	0.663	0.879	0.966	0.992	0.998						
1.3	0.273	0.627	0.857	0.966	0.992	0.998						
1.4	0.247	0.592	0.833	0.946	0.986	0.997	0.999					
1.5	0.223	0.558	0.809	0.934	0.981	0.996	0.999					
1.6	0.202	0.525	0.783	0.921	0.976	0.994	0.999					
1.7	0.183	0.493	0.757	0.907	0.970	0.992	0.998					
1.8	0.165	0.463	0.731	0.891	0.964	0.990	0.997	0.999				
1.9	0.150	0.434	0.704	0.875	0.956	0.987	0.997	0.999				
2.0	0.135	0.406	0.677	0.857	0.947	0.983	0.995	0.999				
2.1	0.122	0.380	0.650	0.839	0.938	0.980	0.994	0.999				
2.2	0.111	0.355	0.623	0.819	0.928	0.975	0.993	0.998				
2.3	0.100	0.331	0.596	0.799	0.916	0.970	0.991	0.997	0.999			
2.4	0.091	0.308	0.570	0.779	0.904	0.964	0.988	0.997	0.999			
2.5	0.082	0.287	0.544	0.758	0.891	0.958	0.986	0.996	0.999			
2.6	0.074	0.267	0.518	0.736	0.877	0.951	0.983	0.995	0.999			
2.7	0.067	0.249	0.494	0.714	0.863	0.943	0.979	0.993	0.998	0.999		
2.8	0.061	0.231	0.469	0.692	0.848	0.935	0.976	0.992	0.998	0.999		
2.9	0.055	0.215	0.446	0.670	0.832	0.926	0.971	0.990	0.997	0.999		
3.0	0.050	0.199	0.423	0.647	0.815	0.916	0.966	0.988	0.996	0.999		
3.2	0.041	0.171	0.380	0.603	0.781	0.895	0.955	0.983	0.994	0.998		
3.4	0.033	0.147	0.340	0.558	0.744	0.871	0.942	0.977	0.992	0.997	0.999	
3.6	0.027	0.126	0.303	0.515	0.706	0.844	0.927	0.969	0.988	0.996	0.999	
3.8	0.022	0.107	0.269	0.473	0.668	0.816	0.909	0.960	0.984	0.994	0.998	0.999
4.0	0.018	0.092	0.238	0.433	0.629	0.785	0.889	0.949	0.979	0.992	0.997	0.999

λ	x	0	1	2	3	4	5	6	7	8	9	10	11	12
4.2	0.015	0.078	0.210	0.395	0.590	0.753	0.867	0.936	0.972	0.989	0.996	0.999	0.999	0.999
4.4	0.012	0.066	0.185	0.359	0.551	0.720	0.844	0.921	0.964	0.985	0.994	0.998	0.999	0.999
4.6	0.010	0.056	0.163	0.326	0.513	0.686	0.818	0.905	0.955	0.980	0.992	0.997	0.999	0.999
4.8	0.008	0.048	0.143	0.294	0.476	0.651	0.791	0.887	0.944	0.975	0.990	0.996	0.999	0.999
5.0	0.007	0.040	0.125	0.265	0.440	0.616	0.762	0.867	0.932	0.968	0.986	0.995	0.998	0.998
5.2	0.006	0.034	0.109	0.238	0.406	0.581	0.732	0.845	0.918	0.960	0.982	0.993	0.997	0.997
5.4	0.005	0.029	0.095	0.213	0.373	0.546	0.702	0.822	0.903	0.951	0.977	0.990	0.996	0.996
5.6	0.004	0.024	0.082	0.191	0.342	0.512	0.670	0.797	0.886	0.941	0.972	0.998	0.995	0.995
5.8	0.003	0.021	0.072	0.170	0.313	0.478	0.638	0.771	0.867	0.929	0.965	0.984	0.993	0.993
6.0	0.002	0.017	0.062	0.151	0.285	0.446	0.606	0.744	0.847	0.916	0.957	0.980	0.991	0.991
6.2	0.002	0.015	0.054	0.134	0.259	0.414	0.574	0.716	0.826	0.902	0.949	0.975	0.989	0.989
6.4	0.002	0.012	0.046	0.119	0.235	0.384	0.542	0.687	0.803	0.886	0.939	0.969	0.986	0.986
6.6	0.001	0.010	0.040	0.105	0.213	0.355	0.511	0.658	0.780	0.869	0.927	0.963	0.982	0.982
6.8	0.001	0.009	0.034	0.093	0.192	0.327	0.480	0.628	0.755	0.850	0.915	0.955	0.978	0.978
7.0	0.001	0.007	0.030	0.082	0.173	0.301	0.450	0.599	0.729	0.830	0.901	0.947	0.973	0.973
7.2	0.001	0.006	0.025	0.072	0.156	0.276	0.420	0.569	0.703	0.810	0.887	0.937	0.967	0.967
7.4	0.001	0.005	0.022	0.063	0.140	0.253	0.392	0.539	0.676	0.788	0.871	0.926	0.961	0.961
7.6	0.001	0.004	0.019	0.055	0.125	0.231	0.365	0.510	0.648	0.765	0.854	0.915	0.954	0.954
7.8	0.004	0.016	0.048	0.112	0.210	0.338	0.481	0.620	0.741	0.835	0.902	0.945	0.945	0.945
8.0	0.003	0.014	0.042	0.100	0.191	0.313	0.453	0.593	0.717	0.816	0.888	0.936	0.936	0.936
8.5	0.002	0.009	0.030	0.074	0.150	0.256	0.386	0.523	0.653	0.764	0.849	0.909	0.909	0.909
9.0	0.001	0.006	0.021	0.055	0.116	0.207	0.324	0.456	0.587	0.706	0.803	0.876	0.876	0.876
9.5	0.001	0.004	0.015	0.040	0.089	0.165	0.269	0.392	0.522	0.645	0.752	0.836	0.836	0.836
10.0	0.003	0.010	0.029	0.067	0.130	0.220	0.333	0.458	0.583	0.697	0.792	0.868	0.868	0.868
11.0	0.001	0.005	0.015	0.038	0.079	0.143	0.232	0.341	0.460	0.579	0.689	0.789	0.789	0.789
12.0	0.001	0.002	0.008	0.020	0.046	0.090	0.155	0.242	0.347	0.462	0.576	0.676	0.676	0.676
13.0	0.001	0.001	0.004	0.011	0.026	0.054	0.100	0.166	0.252	0.353	0.463	0.563	0.563	0.563

λ	x	13	14	15	16	17	18	19	20	21	22	23	24	25
4.2														
4.4														
4.6														
4.8														
5.0	0.999													
5.2	0.999													
5.4	0.999													
5.6	0.998	0.999												
5.8	0.997	0.999												
6.0	0.996	0.999	0.999											
6.2	0.995	0.998	0.999											
6.4	0.994	0.997	0.999											
6.6	0.992	0.997	0.999	0.999										
6.8	0.990	0.996	0.998	0.999										
7.0	0.987	0.994	0.998	0.999										
7.2	0.984	0.993	0.997	0.999	0.999									
7.4	0.980	0.991	0.996	0.998	0.999									
7.6	0.976	0.989	0.996	0.998	0.999									
7.8	0.971	0.986	0.993	0.997	0.999									
8.0	0.966	0.983	0.992	0.996	0.998	0.999								
8.5	0.949	0.973	0.986	0.993	0.997	0.999	0.999							
9.0	0.926	0.959	0.978	0.989	0.995	0.998	0.999	0.999						
9.5	0.898	0.940	0.967	0.982	0.991	0.996	0.998	0.998	0.999					
10.0	0.864	0.917	0.951	0.973	0.986	0.993	0.997	0.997	0.998	0.999				
11.0	0.781	0.854	0.907	0.944	0.968	0.982	0.991	0.995	0.998	0.999	0.999			
12.0	0.682	0.772	0.844	0.899	0.937	0.963	0.979	0.988	0.994	0.997	0.997	0.999	0.999	
13.0	0.573	0.675	0.764	0.835	0.890	0.930	0.957	0.975	0.986	0.992	0.996	0.998	0.999	

λ	x	4	5	6	7	8	9	10	11	12	13	14	15	16
14.0		0.002	0.006	0.014	0.032	0.062	0.109	0.176	0.260	0.358	0.464	0.570	0.669	0.756
15.0		0.001	0.003	0.008	0.018	0.037	0.070	0.118	0.185	0.268	0.363	0.466	0.568	0.664
16.0			0.001	0.004	0.010	0.022	0.043	0.077	0.127	0.193	0.275	0.368	0.467	0.566
17.0			0.001	0.002	0.005	0.013	0.026	0.049	0.085	0.135	0.201	0.281	0.371	0.468
18.0				0.001	0.003	0.007	0.015	0.030	0.055	0.092	0.143	0.208	0.287	0.375
19.0				0.001	0.002	0.004	0.009	0.018	0.035	0.061	0.098	0.150	0.215	0.292
20.0					0.001	0.002	0.005	0.011	0.021	0.039	0.066	0.105	0.157	0.221
21.0						0.001	0.003	0.006	0.013	0.025	0.043	0.072	0.111	0.163
22.0						0.001	0.002	0.004	0.008	0.015	0.028	0.048	0.077	0.117
23.0							0.001	0.002	0.004	0.009	0.017	0.031	0.052	0.082
24.0								0.001	0.003	0.005	0.011	0.020	0.034	0.056
25.0								0.001	0.001	0.003	0.006	0.012	0.022	0.038

λ	x	17	18	19	20	21	22	23	24	25	26	27	28	29
14.0		0.827	0.883	0.923	0.952	0.971	0.983	0.991	0.995	0.997	0.999	0.999		
15.0		0.749	0.819	0.875	0.917	0.947	0.967	0.981	0.989	0.994	0.997	0.998	0.999	
16.0		0.659	0.742	0.812	0.868	0.911	0.942	0.963	0.978	0.987	0.993	0.996	0.998	0.999
17.0		0.564	0.655	0.736	0.805	0.861	0.905	0.937	0.959	0.975	0.985	0.991	0.995	0.997
18.0		0.469	0.562	0.651	0.731	0.799	0.855	0.899	0.932	0.955	0.972	0.983	0.990	0.994
19.0		0.378	0.469	0.561	0.647	0.725	0.793	0.849	0.893	0.927	0.951	0.969	0.980	0.988
20.0		0.297	0.381	0.470	0.559	0.644	0.721	0.787	0.843	0.888	0.922	0.948	0.966	0.978
21.0		0.227	0.302	0.384	0.471	0.558	0.640	0.716	0.782	0.838	0.883	0.917	0.944	0.963
22.0		0.169	0.232	0.306	0.387	0.472	0.556	0.637	0.712	0.777	0.832	0.877	0.913	0.940
23.0		0.123	0.175	0.238	0.310	0.389	0.472	0.555	0.635	0.708	0.772	0.827	0.873	0.908
24.0		0.087	0.128	0.180	0.243	0.314	0.392	0.473	0.554	0.632	0.704	0.768	0.823	0.868
25.0		0.060	0.092	0.134	0.185	0.247	0.318	0.394	0.473	0.553	0.629	0.700	0.763	0.818

λ	x	30	31	32	33	34	35	36	37	38	39	40	41	42
14.0														
15.0														
16.0		0.999												
17.0		0.999	0.999											
18.0		0.997	0.998	0.999										
19.0		0.993	0.996	0.998	0.999	0.999								
20.0		0.987	0.992	0.995	0.997	0.999	0.999							
21.0		0.976	0.985	0.991	0.994	0.997	0.998	0.999	0.999					
22.0		0.959	0.973	0.983	0.989	0.994	0.996	0.998	0.999	0.999				
23.0		0.936	0.956	0.971	0.981	0.988	0.993	0.996	0.997	0.999	0.999			
24.0		0.904	0.932	0.953	0.969	0.979	0.987	0.992	0.995	0.997	0.998	0.999	0.999	
25.0		0.863	0.900	0.929	0.950	0.966	0.978	0.985	0.991	0.994	0.997	0.998	0.999	0.999

APPENDIX C

CHI-SQUARE DISTRIBUTION¹

A tabulation of the probability that χ^2 will be exceeded (α).

ν \ α	.995	.990	.975	.950	.050	.025	.010	.005
1	--	--	--	.004	3.84	5.02	6.63	7.88
2	.01	.02	.05	.10	5.99	7.38	9.21	10.60
3	.07	.11	.22	.35	7.81	9.35	11.34	12.84
4	.21	.30	.48	.71	9.49	11.14	13.28	14.86
5	.41	.55	.83	1.15	11.07	12.83	15.09	16.75
6	.68	.87	1.24	1.64	12.59	14.45	16.81	18.55
7	.99	1.24	1.69	2.17	14.07	16.01	18.48	20.28
8	1.34	1.65	2.18	2.73	15.51	17.53	20.09	21.96
9	1.73	2.09	2.70	3.33	16.92	19.02	21.67	23.59
10	2.16	2.56	3.25	3.94	18.31	20.48	23.21	25.19

¹Chou, Statistical Analysis, p. 775.

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